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Section S17

No.

SOME THOUGHTS
ON THE
TEXTUAL CRITICISM OF THE
NEW TESTAMENT

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NEW TESTAMENT

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TEXTUAL CRITICISM OF THE NEW TESTAMENT.

CHAPTER I.

THE RECENT HISTORY OF NEW TESTAMENT TEXTUAL CRITICISM.

THE publication of Mr. Miller's defence of *The Traditional Text of the Gospels* has moved me to put on paper some thoughts on the criticism of the New Testament text. It seemed like waking up after fifteen years' sleep to find, on looking at the new theological publications, that the controversy, Burgon versus Westcott and Hort, was still raging. Mr. Miller has put Burgon's name on his title-page, and when he does not give his very words he makes so successful an imitation of

his tone and style that we are sometimes at a loss whether the voice is the master's or the disciple's.¹ And yet most readers will feel that they are asked to try again a ruled cause ; and the general verdict is one which I have no desire to reverse. Burgon's work is dominated by the conviction that every word of the Scriptures was dictated by the inspiration of the Holy Spirit ; that it is inconceivable that the Author of such a gift would allow it to become unavailing, and would not providentially interfere to guard it from being corrupted or lost ; that we may therefore rightly believe that He guided His Church through the course of ages to eliminate the errors which the frailty of man had introduced ; and consequently that the text which has been used by the Church for centuries must be accepted as at least substantially correct.²

¹ In the second volume, published since the above was written, it is much more easy to distinguish the parts that claim Burgon's direct authorship.

² I think Burgon's views of the history of the text are fairly represented in the following sentences from Miller's work : "Demonstrable it is that the text of the Gospels

Accordingly Burgon tests the goodness of the ancient MSS. by comparison with the Received Text, and finds that in the passages which admit of comparison A seriously deviates from the Received Text 842 times,

at an early period underwent a process of revision at the hands of men who apparently were as little aware of the foolishness as of the sinfulness of all they did; and that mutilation was their favourite method. And, what is very remarkable, the same kind of infatuation which is observed to attend the commission of crime, and often leads to its detection, is largely recognisable here. But the Eye which never sleeps has watched over the deposit and provided Himself with witnesses" (p. 211).

"The settlement of the text, though mainly made in the fourth century, was not finally accomplished till the eighth century at the earliest; and the later uncials, not the oldest, together with the cursives, express, not singly, not in small batches or companies, but in their main agreement, the decisions which had grown up in the Church" (p. 224).

"We trust to the Church of all the ages as the keeper and witness of Holy Writ; we bow to the teaching of the Holy Ghost as conveyed in all wisdom by facts and evidence, and we are certain that, following no preconceived notions of our own, but led under such guidance, moved by principles so reasonable and comprehensive, and observing rules and instructions appealing to us with such authority, we are in all main respects **STANDING UPON THE ROCK**" (p. 239).

C 1,798, B 2,370, **N** 3,392, and D 4,697 times. It is true that he says that he compares these MSS. with the Received Text merely to show how much they differ among themselves, and that any other standard of comparison would answer the purpose equally well. However that may be, he ranges the merit of these MSS. exactly in the order of their closeness to the *Textus Receptus*. Within a few lines of the passage where he gives the figures just cited he estimates their authority as follows: "By far the most depraved text is that exhibited by Codex D; next to D the most untrustworthy codex is **N**; next in impurity comes B; then the fragmentary Codex C; our own A being beyond all doubt disfigured by the fewest blemishes of any." And Burgon gives an amusing explanation of how it comes to pass that the most ancient MSS. are among the worst. Their antiquity is due to their badness; they were known to be so bad that they were little used, and consequently remained untouched on their shelf, and so have survived when their betters have perished, and

now live only in the transcripts made from them.

I think that the majority of any readers I am likely to have will not require me to state at length my reasons for being unable to accept Burgon's principles, and for feeling no confidence in an investigation conducted with such manifest resolve to bring out a predetermined result. And though some of the points which Burgon's learning and ingenuity have raised perhaps deserve more discussion than his adversaries have been inclined to give them, I feel that in the present state of the controversy there is more profit in speaking about Westcott and Hort's work than about Burgon's.

Westcott and Hort's Greek Testament has been described as an epoch-making book ; and quite as correctly as the same phrase has been applied to the work done by Darwin. Before Darwin's time there had been speculations as to the mutability of species, which gained no general acceptance, because regarded as paradoxical, as resting on no scientific basis, and as apparently irreligious ; but which when syste-

matized by Darwin passed rapidly into the creed of men of science, and into the popular belief of the large outside world of persons interested in scientific speculations. In like manner, Westcott and Hort had many precursors in their assault on the authority of the received printed text, though it was only with respect to isolated passages that that authority was at first challenged. A hundred years before their time Gibbon had made widely known the defective authority of one of its readings—the text of the three heavenly witnesses. It was not disputed that the authority of the printed text must yield to that of MSS., but information as to the latter source of knowledge was very scanty. Any one who cares to look at the now forgotten controversy between Porson and Archdeacon Travis, will find that the controversy was made possible only by the want of information as to MS. evidence; the Archdeacon finding it impossible to believe that editors of the printed Greek Testament could have introduced the disputed text without good MSS. to warrant them.

But as research proceeded it became impossible to maintain this assumption, and the last serious defender of that text, Bishop Burgess, had been driven from the field half a century before Westcott and Hort's time. Still, though it had to be owned that there might be cases in which the evidence is such as to justify the displacement of the readings of the *Textus Receptus*, the feeling long prevailed that such a change involved something of irreverence, and ought not to be made without urgent necessity.

Before this time, however, scholars had come to acknowledge that it was not only that the authority of the printed text must yield to that of MSS., but also that the authority of later MSS., however numerous, must yield to that of more ancient evidence. Lachmann set the example of editing on these principles, but his materials did not suffice to enable him to carry out his work in such a way as to commend his method to general acceptance. If questions of priority were of importance, Tregelles might claim to have been in this

country the founder of the school of criticism to which ascendancy has been given by Westcott and Hort, who followed out their predecessor's methods with larger materials and in a more systematic way. But Tregelles was an isolated worker, and failed to gain any large number of adherents. Tischendorf, whose success in bringing new MSS. to light, and diligence in collating them, gave him more authority as an editor than he deserved, familiarized the public mind with the idea that the *Textus Receptus* must be freely departed from. It became notorious that the result of the new criticism would be to remove from the sacred text several passages which had been hallowed by the veneration of centuries. Yet those whose conservative feelings were shocked by the newly edited text could have no confidence that it rested on a really scientific basis. Tischendorf's vacillations in successive editions were such as to make students wish for a guide better able to choose for them between readings which in respect of antiquity of attestation had equal claims to acceptance, and who, it was

hoped, might reverse some too hasty innovations. It was needful that such a guide should possess sound learning, immense industry, and great sobriety of judgment; and it was welcome news when the desired help was offered by the theological school of the University of Cambridge, of which the great triumvirate Lightfoot, Westcott, and Hort were then the leading representatives; men the obligations of our Church to whom it would be hard to exaggerate. In Germany learned investigations had been made, impugning the authority of our sacred books; and in this country orthodox divines had too often thought it enough to reject their results with outcry, without giving them any real examination. Through a natural reaction many an intelligent student was predisposed to accept the new discoveries with as little examination and as little knowledge, believing that he was ranging himself on the side of learned progressive research against fossilized bigotry. But the Cambridge divines gave the new school of critics battle on their own ground, examining their alleged proofs with perfect

candour, and with learning equal or superior to their own ; and the result was, in the opinion of dispassionate judges, a decided defeat of the destructive school of criticism.

If the leaders of the Cambridge school deserved the gratitude of Churchmen who knew them only by their published works, much more was due to them from those who came within the range of their personal influence. By their honesty, sincerity, piety, zeal, and the absence of all self-seeking, they gained the love as well as the admiration of successive generations of students ; and it is hard to say whether they benefited the Church more by their own works or by the learned scholars whom they trained, and who possibly may still outdo the performance of their masters. Surely these were men to whom the most timidly conservative of theologians might have trusted the work of textual revision in full confidence that its results would be such as they could gladly accept. So it was all the more surprising when these critics, who, with regard to the authority of the books, belong to the conservative school, proved to be,

in respect of the criticism of the text, strongly radical and revolutionary. Authorities which Lachmann had admitted into his scanty list were depressed to an inferior place ; readings which Tischendorf had received into his text were bracketed or removed altogether. Possibly it may be found on investigation that the strict orthodoxy of the Revisers had something to do with the stringency of their conditions for admission into their text, and that critics who ascribe less authority to the very words of the sacred writers may be more lenient in their acknowledgment of a claim to authority of the kind.

However this may be, it was from the conservative side that a storm of opposition arose which owed something of its violence to the fact that some of the most startling of the results of the new revision were made known without any explanation of the system through which these results were arrived at. It was through the publication of the Revised Version of the New Testament that English readers became generally aware of the exact degree

to which their English New Testament was liable to be affected by learned criticism of the Greek text. Westcott and Hort were members of the Committee which prepared the Revised Version, and on the question of various readings they exercised a predominating influence. It was a study to which they had devoted their whole lives, while more than half of their brother members of the Committee had given no special attention to the subject, and could not without immodesty dissent from critics of such eminence.

My countryman, Dr. Hort, was a man of *perfervidum ingenium*, who held his opinions with an intensity of conviction which he could not fail to communicate to those who came in contact with him, while his singular skill as an advocate enabled him with small difficulty to dissipate all objections to his own views. I have often admired the remarkable independence of judgment exhibited by his colleague, Westcott, who, on several occasions, has expressed inability to adopt decisions of Hort's ; knowing, as I do, how difficult it was for any one to come within

the sphere of his influence (not to say to carry on work in conjunction with him) without being made to adopt all his conclusions. As in what follows I shall have to criticise some of Dr. Hort's results, I must at the outset express my love and admiration for the man, of whose friendship I have been proud, and for whose readiness to give help and information when asked for I have often had occasion to be grateful. The admiration which our necessarily limited intercourse inspired has been greatly increased by the fascinating biography in which his son has judiciously allowed him principally to reveal his own character by his letters. He exhibits such a sincere love of truth, such a multiplicity of interests, such a determination to leave no scrap of time unemployed, that in his too short life he must have had twice as many hours of useful work as most other men of the same age. If there were any master to whose words I should be content to swear, I could desire no better guide, and I feel that there is a special perverseness in differing with him on points on which he could speak with

infinitely more authority than I. But, lover of truth as he was, he was never desirous that his opinion should be accepted unquestioned. We had some small differences of opinion when he was alive, in which neither could he convince me, nor I him; and I feel that I can exercise equal freedom now. Ἐν ἄλλοις μὲν πολλοῖς ἀποδέχομαι καὶ ἀγαπῶ τὸν ἄνθρωπον, τῆς τε πίστεως καὶ τῆς φιλοπονίας καὶ τῆς ἐν ταῖς γραφαῖς διατριβῆς, καὶ πάνυ δι' αἰδοῦς ἄγω τὸν ἄνθρωπον, ταύτη μᾶλλον ἢ προανεπαύσατο. Ἀλλὰ φίλη γὰρ καὶ προτιμωτάτη πάντων ἡ ἀλήθεια, ἐπαιεῖν τε χρῆ καὶ συναιεῖν ἀφθόνως, εἴ τι ὀρθῶς λέγοιτο, ἐξετάζειν τε καὶ διευθύνειν εἴ τι μὴ φαίνοιτο ὑγιῶς ἀναγεγραμμένον. Καὶ πρὸς μὲν παρόντα καὶ ψιλῶ λόγῳ δογματίζοντα αὐτάρκης ἦν ἂν ἡ ἀγράφος ὁμιλία, γραφῆς δὲ ἐκκειμένης, ὡς δοκεῖ τισὶ πιθανωτάτης, ἀναγκαῖον καὶ ἡμᾶς, ὡς πρὸς παρόντα τὸν ἀδελφὸν ἡμῶν διαλεχθῆναι.¹

I return from this digression to the subject of which I had been speaking—*viz.* the inconvenience arising from the fact that Westcott

¹ Euseb., *H. E.*, vii. 24.

and Hort's results became known some time before the public had the means of knowing the process by which their conclusions had been arrived at. The company of the New Testament Revisers were indeed privately supplied with instalments of Westcott and Hort's Greek text¹ as their work required them. But that text did not come into the hands of the public until a little after the appearance of the Revised English Testament ; and those who took offence at the omission or alteration of certain familiar phrases or texts had to wait some time longer before they had the opportunity of learning, from Dr. Hort's Introduction, the principles on which the work of text-revision had been conducted.

The result was that, in the controversies which followed the appearance of the new text, there was a good deal of fighting in the dark, the combatants on both sides often contradicting

¹ In speaking of the Greek text I use the abbreviation WH; when I refer to the Introduction I generally say Hort, that being his composition, though, no doubt, his colleague willingly shares the responsibility.

what had not been asserted, and laboriously proving what had not been denied. It is well remembered what a vehement assault was made by Dean Burgon in an article in the *Quarterly* on the Revised English New Testament when it appeared ; followed up by other articles on the publication of WH's Greek text. Indeed, a few years before the appearance of that text, Burgon had by anticipation attempted to demolish one of its important conclusions—*viz.* the rejection of the closing verses of St. Mark's Gospel. In this controversy Burgon took much pains to prove that passages which WH reject were acknowledged by Epiphanius, Basil, Chrysostom, and a whole host of other authorities. But he might have spared himself much of this trouble if he had known how freely the facts which he brings forward were acknowledged by WH. They allow that the text whose authority they assail had acquired predominance in Chrysostom's time, and had taken its origin probably a century earlier ; consequently the great MS. known as Codex A which Lachmann

had used in moulding his text was a witness too late to be admitted by WH as having authority to affect their conclusions.

On the other hand, Burgon's attack on WH enlisted on the side of the Cambridge divines a certain number of critics with more zeal than accurate knowledge of the controversy. Burgon's way of estimating the goodness of the ancient MSS. by the amount of their deviation from Lloyd's Greek Testament certainly left him without any right to complain if his readers concluded that he counted the common printed text the model of perfection, and regarded Bishop Lloyd's Greek Testament as the authority which gave the closest representation of the evangelic autographs. Nevertheless it is true that the criticisms were beside the mark, when Burgon's opponents took pains to show on what slender authority the common printed text rests, having taken its origin in a haphazard way from the edition at first hastily scrambled together by Erasmus, and afterwards imperfectly amended by himself, by Stephens, and other editors on a quite insufficient collation

or even knowledge of MSS. It must be borne in mind that the "received" text is such as it is because, as Hort freely acknowledges, it was substantially the received text of the MSS. current at the date when it was first printed and for many centuries before that date. The real point at issue between Burgon and WH is not what credit is due to the New Testament text of Erasmus, but what to the text used by St. Chrysostom.

This question evidently raises another : what credit is due to the authorities earlier than Chrysostom? Concerning these, Burgon and WH form widely different estimates ; but there has been a good deal of misconception as to the ground of their difference. With WH the highest MS. authority is the accordance of the Vatican MS. (B) with the Sinaitic (Ⲛ). Hort expresses his belief (p. 225) (1) that readings of Ⲛ B should be accepted as the true readings until strong internal evidence is found to the contrary ; and (2) that no readings of Ⲛ B can safely be rejected absolutely, though it is sometimes right to place them only on an

alternative footing, especially when they receive no support from versions or Fathers. Burgon, on the contrary, maintains that with the single exception of D, which exhibits the wildest text of all, the two MSS. honoured by WH are the most corrupt. He assures his readers,¹ "without a particle of hesitation, that **ⲛ B D** are *three of the most scandalously corrupt copies extant, exhibit the most shamefully mutilated texts* which are anywhere to be met with—have become by whatever process (for their history is wholly unknown) depositories of the largest amount of *fabricated readings, ancient blunders, and intentional perversions of truth* which are discernible in any known copies of the Word of God" (*Revision Revised*, p. 16).

On reading this condemnation of the oldest of our MSS. as containing a text far less pure than that current in the sixteenth century, it was a natural inference that the great difference between Hort and Burgon was that the one founded his text on the oldest author-

¹ The italics are Burgon's.

ities and the other on the latest. But it is by no means true that Hort made the age of a MS. the criterion of its goodness; on the contrary, he is quite at one with Scrivener in pronouncing some corruptions of the text to be as early as the second century. Some of my hesitations in following Hort are on points on which he agrees with Burgon; and I have my doubts whether both the one and the other do not pay too little respect to the antiquity of testimony which conflicts with their canons of probability. At any rate, we evidently cannot determine the dispute between Burgon and Hort as summarily as we might if the controversy were—Which is more likely to be right, the oldest authority or the latest?

The questions raised by Westcott and Hort are such as cannot be decided offhand, nor is any one who is not an expert entitled to pass judgment on them. Scrivener's meritorious labours have done much to popularize the science of Biblical criticism. An ordinary well-informed clergyman may now be expected to know how to use a critical edition of the New Testa-

ment, and may easily suppose himself qualified to give some independent judgment of his own. By referring to Tregelles or Tischendorf he can learn what MSS. favour the one reading, what the other, and what Fathers quote the text in the first way, what in the second, and he may deem himself able to judge which reading makes the better sense. Yet the judgment formed by a person who tries to balance the evidence, external and internal, with regard to an isolated text may be quite different from that of an editor who has worked through the whole New Testament. In the course of such a work each witness must have come hundreds of times before the judge, who cannot help forming an opinion as to his character and credibility. If in a multitude of cases he finds one witness, or group of witnesses, attesting what he persuades himself to be the true reading, while the other is guilty of careless or licentious variations from it, it is inevitable that he will be disposed to believe the former rather than the latter, even in cases where, if the witnesses had come

before him as entire strangers, he might have thought that the report of the latter had more probability to recommend it.

Any one then who dips into textual criticism in an unsystematic way, looking only into the evidence with regard to a few selected texts, must feel timid when his judgment is opposed to that of an expert. Suppose that we were inquiring into the real facts of some occurrence, and that two schoolboys gave us contradictory accounts of it, we might exercise our own judgment as to which story seemed more probable. But if the schoolmaster of the two boys came up and told us that the one was a notorious liar and the other a boy of honour on whose word we might implicitly trust, our judgment might be in a moment reversed, and we should believe the less probable account on the testimony of the more credible witness.

Thus when WH's edition appeared, though there were several of their results which startled me, I felt that it would be immodest to oppose my prepossessions to the deliberate judgment

of men who had given as many years to the study as I had given months. Yet were I to form any opinion on the subject, I could not help differing from some who had every right to count as better judges than myself; for experts were ranged on opposite sides. Burgon, who rejected Hort's results with angry vituperation, was well entitled to rank as an expert, for he was familiar with all varieties of readings, and had probably handled and collated very many more MSS. than either Westcott or Hort. Burgon declares that "no person is competent to pronounce concerning the merits or demerits of cursive copies of the Gospel, who has not himself collated with great exactness at least a few of them," "of which labour if a man has had no experience at all he must submit to be assured that he really has no right to express himself confidently on this subject-matter." Scrivener, the scholar who was at the time at the head of all English collators of New Testament MSS., though by no means as bigoted a partisan of the Received Text as

Burton, yet in a number of important points sided with him against the new theories. Who then was to decide when doctors disagreed? Which experts were we to follow when experts were ranged on opposite sides?

The case, however, is one which constantly happens in courts of justice, when scientific men give evidence on opposite sides; and judge and jury are forced to decide as best they can which they will believe, sure that in either case they must reject the opinion of persons entitled to speak with much higher authority than they. And I found it hard to maintain the modest attitude of suspension of judgment, when I saw sides taken and opinions confidently expressed on this subject of Biblical criticism by a number of people who, I was sure, knew no more about the matter than myself, or, as Socrates might put it, who knew less, since they did not sufficiently know their own ignorance.

On the one hand it is easy to understand how strong was the inclination with many to reject the new edition with little examination. In the first place, hostility was excited by its omissions.

The reader found passages rejected as no part of the genuine apostolic record, which had been endeared by the veneration of the Catholic Church for centuries. St. Mark's Gospel loses more than half its concluding chapter; from St. Luke's Gospel we are taught to erase the story of the Bloody Sweat, and the divine words on the cross, "Father, forgive them; for they know not what they do." That is to say, we are not forbidden to believe that our Lord spoke these words, but only that we have Luke's authority for so believing. In these and several other cases of omission, a student who examines the evidence for himself, without having mastered WH's principles of dealing with it, would be likely to think that a bad reading had been adopted in the teeth of evidence, overpowering both in respect of the number and the antiquity of the witnesses in favour of the reading which the Church for many centuries had received. Nay, it would seem as if in the judgment of the new editors *any* evidence was good enough to justify an omission. There is no authority which, when it stands alone,

finds less favour in the eyes of these editors than that of Codex D and of Western MSS. generally. Indeed, with them to describe a reading as Western is a note of contumely. Yet, when D omits what is attested by a consensus of other authorities, including those which WH count the highest, they are persuaded that this time D is in the right, and pronounce the reading to be a case of "Western non-interpolation."

But even more repulsive to conservative instincts was the number of cases in which these editors attribute to the Evangelists themselves erroneous statements which their predecessors had regarded as copyists' blunders. There was indeed but a little rhetorical exaggeration in the statement that the canon of these editors was that Codex B was infallible and that the Evangelists were not. Nay, it seemed as if Hort regarded it as a note of genuineness if a reading implies error on the part of a sacred writer. In one case (Matt. xiii. 35), where B unites with every extant MS. but one in giving a text free from error, Hort is willing

to accept the testimony of a single MS. that Matthew ascribed to Isaiah a passage really taken from the Psalms. This apparently perverse decision was suggested by the no doubt true principle that if an intelligent copyist found in his archetype what seemed a plain mistake he would be under a temptation to correct it in his transcript, whereas he would be very unlikely to impute to the sacred writer a mistake which he had not committed. Consequently the presumption would be that blunders had been made rather by the original author than by the transcribers ; and that a text free from blunders would be likely to have owed its correctness to its copyists. At all events, there was much in the new edition to stagger even one who takes a very liberal view of the possibility of error in the evangelic narrative.

I will not lay over-much stress on such cases as that WH make St. Mark say, not that David ate the shewbread in the time of Abiathar, who was afterwards high priest, but in the high priesthood of Abiathar, which was not the case ; that the girl who danced before

Herod was not, as Josephus and other authorities tell us, the daughter of Herodias by a former husband, but Herod's own daughter, Herodias; that it makes St. Luke call the miraculous darkness at the Crucifixion an eclipse of the sun, a thing impossible at the time of full moon. For myself, though my prepossessions certainly would have led me to expect absolute accuracy, I own that my expectations are no rule to measure the degree in which the Holy Spirit would interfere to guard the Evangelists from error, and that in the absence of any assertion, either by the Evangelist himself, or by any other sacred writer for him, that such a lapse on his part was absolutely impossible, I must allow my belief to be determined by evidence. If it can be proved that St. Mark said "the high priesthood of Abiathar," when in strict accuracy he ought to have said "the time of Abiathar," that need not one whit affect the credence we give to his testimony concerning the miraculous life and death of our Lord.

Yet if we admit it to be possible that an Evangelist might come short of perfect accu-

racy in his references to Old Testament history, at least we should expect him to know his Old Testament fairly well, and not to make *bad* mistakes. But in the very first chapter of St. Matthew, according to WH, the Evangelist makes the name of one King of Judah Asaph instead of Asa, and of another Amos instead of Amon. If a Sunday-school child thus mixed up the names of two prophets and two kings, we should not be satisfied with his answering ; and it seems hard to believe that St. Matthew knew no better.¹ And an unskilled critic might easily imagine that there was a preponderance of MS. evidence in favour of the Received Text.

Enough has been said to explain why there should have been many prepared to take sides against WH ; yet with many others of those unable to pronounce a skilled judgment of their own, the apparent strength of the case for the Received Text was felt to be a reason for accepting the unfavourable verdict of the recent

¹ The confusion was not likely to be made by any one who used a Hebrew Bible, in which the first letters of Amos and Amon are different.

editors. For they were men of known sincere piety and orthodoxy, who would not have accepted conclusions at first sight so disagreeable unless they had felt themselves constrained to follow honestly to its legitimate end the path of scientific investigation. It was to be expected that this honest boldness should provoke an outcry from old-fashioned critics with whom the claims of custom find more favour than those of truth; but such outcry might safely be disregarded. The personal qualifications of the new editors were such that a student could feel it safe to follow their guidance, and consider that he was ranging himself on the side of enlightened progress against old-fashioned bigotry.

This confidence was increased and felt to be justified as acquaintance was gained with the principles on which the new editors had worked. In the progress of Biblical criticism, naturally the work of collecting the evidence came before that of putting it in order and weighing the value of each part; and it is not to be assumed that those who had been most

successful in gathering the materials would also be the persons able to make the best use of them. WH did not count it their special work to add to the abundant store of materials which their predecessors had brought together ; but, sensible that authorities must not be counted but weighed, they set themselves to test the different testimonies, examine into their independence, and try to appreciate their relative value. The scientific character of their methods was soon perceived, and the difference between their predecessors and them seemed to be like that between the old school of pre-scientific historians, who, without discrimination, incorporated in their narrative all that chroniclers had told, and the modern method of first carefully weighing the trustworthiness of each authority, and building nothing on the statements of those which were found to be unworthy of credit. Even if some of the decisions made by these editors may hereafter be modified or even rejected, their methods must be admired and imitated.

For myself, though I carried a systematic

examination of the text but a little way, yet, as far as I went, I found myself repeatedly making the same decisions as WH, even in cases where they were opposed to my prepossessions. For example, in the case just mentioned—Asa or Asaph—I found that if I looked only to the earliest witnesses there was a decided preponderance for the faulty reading; the best witnesses on the other side making themselves suspected by having in other places tampered with the text in order to remove what seemed to them an inaccuracy.¹ So I am disposed to believe that a New Testament editor, bound to produce the text as given by the most ancient witnesses, would feel himself constrained to edit the faulty reading, leaving it for commentators to account for the error, whether by attributing it to the carelessness of one of the first transcribers, who produced the parent of what are now the most ancient copies, or to an error in the ancient family genealogy

¹ I refer in particular to the case (Matt. xxvii. 9) where, even according to the Received Text, the Evangelist ascribes to Jeremiah a prophecy really due to Zechariah.

which the Evangelist faithfully copied when he incorporated it with his Gospel.

Yet great as has been my veneration for Hort, and my admiration of the good work that he has done, I have never been able to feel that his work was final, and I have disliked the servility with which his history of the text has been accepted, and even his nomenclature adopted, as if now the last word had been said on the subject of New Testament criticism. Not that I expect Burgon ever to be set on his legs again, but I think that in Hort's work will be found some rash decisions which calmer followers will regard as at least doubtful. There is no immodesty if one who is not an expert himself attempts to form some judgment of his own between the views of those who are. It is, as has been said, what judges and juries are daily obliged to do. If experts on opposite sides use arguments, outsiders can form an opinion which is the more cautious and logical reasoner. If Burgon's violence and confidence carried many with him, many more were repelled by the suspicions raised by his deficiency

in calm impartiality of judgment. That which gained Hort so many adherents had some adverse influence with myself—I mean his extreme cleverness as an advocate ; for I have felt as if there were no reading so improbable that he could not give good reasons for thinking it to be the only genuine. He is in his way even more confident than Burgon, and is just as resolved not to allow his antagonists to score a single point. This has generated in my mind a certain sympathy for the witnesses whose testimony he rejects, who seem to me to be hardly dealt with. I do not say they do not deserve the bad treatment they get. Dr. Hort knew them much better than I ; and the likings and dislikings which he exhibits are doubtless not prejudices taken up before investigation, but impressions produced by investigation. All the same, a reader who has not gone through the same process is not always prepared to sympathize with him. I said a little before that we should readily accept the report of a school-master as to the character of his boys. Still if we heard him snub one boy every time he

opened his lips, and bid him hold his tongue for a liar, while he swallowed the most improbable tales on the word of another, an uneasy suspicion might come over us: Does this good man make favourites? Is it quite certain that he does not allow one boy to humbug him, and that he is not thus led to be unjust to the other?

What made me hesitate in following Hort was a certain exaggeration of judgment. When he has satisfied himself that of two alternative views one is much more probable than the other, he dismisses the less probable as absolutely unworthy of consideration, the more probable as so demonstrated as to afford a firm foundation for further theories. Yet when propositions, each separately no more than probable, are combined, the resulting conclusion has but a lower degree of probability. Notwithstanding these causes for doubt, I know that the divinations of an expert will not always bear to be tested by strict rules of logic; and that an expert may often have just confidence in the certainty of judgments of which, when he tries

to give an account to an outsider, his reasons may seem to come much short of logical proof. Hort's conclusions, therefore, cannot be overthrown until they have been tested by another expert who shall have devoted to the study an equal amount of skilled labour. And though I have decided not to carry to the grave with me doubts that I have long entertained as to the soundness of some of his decisions, yet I express those doubts with great timidity, and not as asking a reversal of the rulings which I have not seen my way to accept, but merely as requesting a new trial by well-qualified judges.

The fact is that the foundations of WH's system are buried out of sight of ordinary readers of their work. Their theories are based on immense inductions, in the course of which they must, with enormous labour, have tabulated comparative lists of the peculiarities of MSS., or groups of MSS. These tables no doubt have been preserved, and will probably be available for use by any competent person who may hereafter take up the work of New Testament editing; but they would

manifestly be too voluminous for incorporation in Dr. Hort's Introduction. There he had to content himself with giving specimens of his proofs. These specimens were challenged by his opponents, and plausible reasons were given for not accepting them as sufficient proofs of what they were intended to establish. And yet, supposing that we tried to form an independent judgment on this controversy, it would be very unsafe to reject Hort's conclusions, even if it appeared to us that in these particular cases his opponents had the best of the argument, because we have reason for thinking that the evidence that he produced is small in comparison with that which he kept in reserve. I have, therefore, myself been content to accept on faith, at least provisionally, conclusions of Hort's, even when the arguments which he advanced in support of them left in my mind room for a good deal of doubt.

Nevertheless, if no one but an expert is entitled to pass an unfavourable judgment on WH's work, it follows conversely that the adherence of those who are not experts must be

founded rather on faith than on knowledge. On this account I am not deterred by the general adoption of WH's decisions from expressing my opinion that their work has been too readily accepted as final, and that students have been too willing to accept as their motto, "Rest and be thankful." There is no such enemy to progress as the belief that perfection has been already attained. Therefore I think it will be more useful, if, instead of dwelling on the excellencies of WH's work, I state, with unfeigned modesty, doubts as to some of their decisions, which I have long entertained, and which the progress of years has rather confirmed than dissipated. If what I write has no other use, it may perhaps direct the attention of some future investigator to points on which WH's positions need to be strengthened if not corrected.

CHAPTER II.

WESTCOTT AND HORT'S NOMENCLATURE.

I HAVE already intimated my belief that WH have been quite successful in refuting Burgon's and Miller's idea that the omissions of the Vatican MS. are to be accounted for by the suppositions that its transcriber was abnormally careless, that he was an Arian, or some other kind of heretic, and that his work was therefore in his own generation regarded as unfit for use. I believe, on the contrary, that its type of text had the approval of two of the best critics—Origen and Eusebius; that it is as old as the third century; and, if I cannot quite commit myself to Hort's opinion that its antiquity may be extended to the second, at least I do not venture to contradict it. But we are still a long way from the doctrine that this type is to be accepted as

representing the evangelic autographs. I have elsewhere expressed the opinion that the project of getting back to the autographs is far too ambitious. Predecessors of WH had aimed at restoring a fourth-century text, that is to say, a text free from all later corruptions, and in carrying out this design they could be on perfectly firm ground. They could hold fast to the rule of preferring ancient authorities to modern; admitting readings which had attestation prior to the fourth century in preference to those which had not. But WH could not be content to limit their investigations to the fourth century, and when they went farther back they could find no halting-place short of the autographs. The result has been that they have had quite to abandon the having regard, in their preferences, to the antiquity of testimony. Hosts of readings which they reject have much more ancient attestation than those which they prefer. But evidently something is detracted from the certainty of our conclusions if our canons of probability lead

us to prefer a later authority to an earlier. Dr. Hort deprecates the forming a judgment of our own on isolated texts, a method which gives too much influence to the subjectivity of the inquirer, and can therefore not be trusted to produce uniformity of results. His method is to make first a provisional examination; and if we find in a certain number of cases some authorities giving the reading that we can see to be clearly right, and others giving the wrong one, then to follow the former in other cases. In this way the influence of subjectivity is very much diminished, but it is not altogether eliminated; for it is our own judgment as to what readings in the selected cases are clearly right or clearly wrong that rules our decision, not only in these cases, but in all others having similar attestation. It is very likely that a good critic of the nineteenth century can form a much better judgment as to what an evangelist of the first century would have written, than the critic of the fourth century, who, according to WH, constituted the *Textus Receptus*. Yet there

is a possibility that this assumption may be erroneous, and therefore the results to which it leads must be accepted, not as certain, but only as probable; and therefore such as must be abandoned if stronger opposing probabilities should present themselves. I should have thought it unnecessary to state anything so obvious as that the problem which WH have set themselves is one that admits no more than a probable solution, but that my quarrel with them much more seldom arises from unwillingness to accept their decisions as probable, than from reluctance to acknowledge them as demonstrated facts.

I have said already that, owing to the impossibility of laying the entire evidence before the ordinary reader, he can do no better than acquiesce in WH's decisions, even though he may at times have misgivings. But there is a second reason why criticism of their results is difficult—namely, that their whole tone and method is that of teachers instructing disciples, not that of addressing persons capable of forming an independent judgment. In this, perhaps,

they followed established Cambridge methods. In the exposition of mathematical theorems the course I liked best was to follow the progress of discovery, beginning with the problem which first stirred inquiry, going on to the questions to which the investigation of it gave rise, and finishing with the general principle ultimately arrived at. In Cambridge books, in my early days, little note was taken of history ; and the student was merely furnished with a proof of the final result, which he could get by heart and reproduce on an examination paper when called on to do so. In Hort's exposition the student is not taken with him along the path that he himself had followed ; he must start with the acceptance of the final result. Consequently one of the first things at which I took umbrage in WH's exposition was the question-begging nomenclature. A scientific nomenclature ought to be neutral ; it ought to aim at simply representing the facts without assuming the truth of any theory about them. The first instance I have to mention is one to which I object rather on theoretical grounds,

than because it did any practical mischief. Before WH's edition appeared there had been dispute among New Testament critics as to the value of the then recently discovered Syriac Version published by Dr. Cureton, which some conservative critics had described with very depreciatory epithets, while Cureton himself was willing to accept it as representing the original Aramaic St. Matthew. WH were quite within their province in making a ruling on this controversy; but I think they went outside it when they disturbed the former notation for the Curetonian and Peshitto Versions, which was on scientific grounds quite unobjectionable, since it presupposed no theory as to the dates of the versions; and when they taught their disciples to call the one "Vetus" and the other "Vulgata." Very probably their opinion as to the relative age of the two versions is correct; for such evidence as has since come to light favours their decision: but suppose recent discoveries had opposed it, why should we be committed to a faulty nomenclature? That the Curetonian Version is old I have no doubt:

that it is older than the Peshitto is not proved by historic evidence, but only by the establishment of Hort's theory concerning the growth of the text, and therefore I considered that in expounding his theory it was premature to use a nomenclature which assumed that its truth had been established. I may say, however, that in the absolute dearth of trustworthy historic information as to Syriac versions during the first three centuries, it is wise to refrain from any positive affirmation on the subject. My private opinion is that the version which Hort calls *Vetus* is as old as Tatian, a man who, I am disposed to believe, like Origen, lost through some wildness of speculation the gratitude from the Church which his services to the cause of the Gospel deserved. On his return from Rome he would have brought back MSS. with him, and thus the affinities of the Syriac Gospel with the Western text could be accounted for. But what kind of text was current among Syriac-speaking people before Tatian's time, is a point on which we have not materials to enable us to speak with any confidence. And I dislike the

name "Vetus," because it seems to imply a ruling on this point, and to teach us to regard a version of the Curetonian type as *the* old Syriac translation which had been accepted from the first. If Zahn's theory be correct that at first at the weekly meetings the Gospels were read in Greek and then interpreted into the vernacular by the reader, variations between the Greek MSS. used in different places might reproduce themselves in Syriac, while yet a good deal of the phraseology might have become stereotyped by traditional usage.

However, I willingly concede to experts the right to hold strong opinions on the relative antiquity of Syriac versions; and I should not have thought it worth while to notice this instance of Hort's innovations in nomenclature if I had not to speak of another case where I am persuaded that his innovations had the mischievous effect upon thought which a wrong use of words is apt to produce.

In recognizing three ancient types of text, Hort followed Griesbach, who called them Constantinopolitan, Western, and Alexandrian.

The Constantinopolitan, the most modern of Griesbach's three, as its name indicates, is substantially what Hort calls Syrian; and we need not quarrel about the name,¹ since it may well be believed that Constantinople had derived much of its scholarship from Antioch. It would have been very excusable if Hort had refused to accept from Griesbach the title "Western," because this type of text is by no means confined to the West. In fact, Hort states (p. 113) that the text of all those writers not connected with Alexandria who have left considerable remains is substantially "Western." And he states (p. 127) that the only extant patristic

¹ My chief objection to the name is that it is open to a confusion between Syrian and Syriac. Thus when Hort, as he often does, describes a reading as Western and Syrian, the reader is in danger of supposing the meaning to be that the Western reading agrees with that of the ancient Syriac versions, whereas what he really means is that the Western reading has been adopted by the *Textus Receptus*, which he calls Syrian. I think Hort's idea would have been better expressed by the word "Antiochian"; or if it were thought impossible to gain acceptance for a word of five syllables in modern English, Antioch might have been used as an adjective.

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writings which to any considerable extent support pre-Syrian non-Western readings are all connected with Alexandria—*viz.* the remains of Clement and Origen, the fragments of Dionysius and Peter of Alexandria, and in a certain measure the works of Eusebius, who was deeply versed in the theological literature of Alexandria. Add to this that among the great versions which date from the earliest centuries it is only the two of Upper and Lower Egypt that can be pronounced extensively non-Western. Thus it would appear that the name “Non-Alexandrian” for this type of text would be more accurate than “Western”; but Hort pleads that to change a name now in use for a century would lead to confusion.

It would be more easy to accept this excuse if Hort were equally conservative in other cases; but while he refuses to alter the name “Western,” notwithstanding that he owns that readings of this class were current in the East as well as in the West, and probably to a great extent had originated there, he refuses to retain the equally old use of the word “Alexandrian”

in the ground that non-Western readings were not confined to Alexandria. The explanation of this inconsistency I believe to be found in the aim which, as I have already said, WE set before them, namely, to restore the apostolic autographs. When they had recovered as they believed an original text, the parent of all the others, it seemed to them disavouring to it (see Hort, p. 190) to give it a local name, and hence they called it 'neutral,' as being free from later corruptions, whether introduced at Rome or at Alexandria. I count this to be a question-begging name. A geographical name is not question-begging. If it ~~was~~ possible for us to make with certainty a geographical distribution of MSS., so as to determine which text was used at Rome, which at Antioch, which at Alexandria, this would be a valuable piece of knowledge. Critics would be perfectly free to examine which text was most worthy of confidence, while they would find in the agreement of all three the very strongest claim to acceptance. But a name founded on the quality of the text presupposes that the text has been

examined, and a decision pronounced on it which those who adopt the nomenclature cannot consistently reverse. The name "neutral" presupposes the establishment of WH's theory that all additions and alterations in this neutral text are due to later corruptions. But little mischief would have been done by the substitution of a new name for the old name "Alexandrian," if Hort had not, in order to shield his "neutral" text from the danger of being dishonoured by the epithet "Alexandrian," appropriated that title to another family of readings. He had been alive to the danger of confusion likely to arise if he departed from the established use of the word "Western"; but far greater confusion has arisen from his use of the word "Alexandrian" in a sense in which nobody before him had employed it. I have already quoted Hort's acknowledgment of the Alexandrian character of the attestation of pre-Syrian non-Western readings. This is as true of the readings which Hort calls "neutral" as of those to which he limits the name "Alexandrian." The latter name he confines to

a class of readings "apparently originating in Alexandria, and limited in their early range." "The variations have usually more to do with language than of matter, and are marked by an effort after correctness of phrase." But if we want a more precise answer to the question what Hort means by "Alexandrian," we shall not be far wrong in saying, those readings which are Alexandrian in their origin and are not recognized by Codex B. It follows at once, not as a thing proved by evidence, but as a logical consequence of the definition, that B is neutral as being free, not only from Western, but also from Alexandrian readings. I consider that it is not scientific to stereotype a theory by a nomenclature until the theory has been established beyond reach of controversy. If WH have said the last word about New Testament criticism, we shall do well to adopt their nomenclature; but if it is to be open to us to examine the foundations of their theory, the first step to progress must be the abandonment of the fettering names—in particular the word "neutral."

I strongly feel that Hort would have done better if he had left the old nomenclature undisturbed, and distinguished his neutral text from that which he calls "Alexandrian" by the names "early Alexandrian" and "later Alexandrian."¹ Names will not alter facts, though they may enable us to shut our eyes to them; and whatever names be used, the fact remains that in early times non-Western readings were limited in their range of prevalence. I do not think I underrate the immense service which WH have rendered to Biblical criticism, if I express my opinion that what they have restored is not the text of the original evangelic autographs, but the text of a MS. which came very early to Alexandria—probably in the third century and possibly before the end of the second. To this result Hort was naturally led by his method—*viz.* to take certain selected

¹ Hort (p. 171) makes the suggestive remark that documents which have most Alexandrian have also most neutral readings. It is a little surprising that he did not draw the obvious inference that this is because the documents which contain the neutral readings *are* Alexandrian.

cases, and having in these cases determined the correct reading, to regard the authorities which gave that reading as entitled to preference in all other cases. Now there is no early information about readings more valuable than that given by Origen, who notes several variations of reading and declares his preferences. Such readings are most suitable for testing purposes. Naturally Hort regarded those MSS. as most trustworthy which give the readings recognized by Origen; and these no doubt were the readings which in the third century were most preferred at Alexandria. Thus Hort's method inevitably led to the exclusive adoption of the Alexandrian text.

If it were not that Hort considered any local name to be dishonouring to the text which he prefers, there is certainly no note of disparagement in the epithet "Alexandrian," for, as Hort remarks, it would not be surprising that a purer text should be preserved at Alexandria than in any other Church; for there, owing to the proximity of an exact grammatical school, a more than usual watchfulness over the writings

of Apostles and apostolic men might be expected to be suggested and kept alive. If there were now disinterred from an Egyptian tomb a second or third-century New Testament MS., it would be regarded as an authority superior to any now accessible to us; and I ought not to be thought wanting in appreciation of the merits of WH's work, when I hold that by their successful restoration of an early Alexandrian text they have conferred on Biblical criticism a benefit of the same kind.

But one evil consequence has resulted from their refusal to give their text a local name. However high the authority we might ascribe to an ancient Alexandrian MS., we should not believe that it was infallible. If Alexandrians made such a claim, they would have exposed themselves to the questions—What! came the Word of God out from you? or came it unto you only? Alexandria was not the only city to which the Gospel came; and if we found in the Alexandrian MS. a reading which had all the appearance of being erroneous, we should think it reasonable to

inquire, Was the same reading found in the text used by the Church of Rome, which must have been in possession of many MSS. that had come to it quite independently of Alexandria? But if we imagined that we were in possession of the apostolic autographs, or at least of the nearest approach that can now be made to them, we should naturally set aside all local variations with small examination. This is the attitude which Hort takes towards Western readings. In his eyes a reading is condemned at once if he can describe it as "Western" or "Western and Syrian." It may be very ancient, very interesting, a very fine tradition; but it must not be thought of as part of the Gospel. He even seems to regard the Western scribes as such inveterate liars as only to tell the truth by accident.¹ If his neutral text presents an

¹ Hort's method of testing the goodness of groups of MSS. has in some cases led him to the curious result that a group of two or three MSS. which ordinarily is found to give correct readings becomes untrustworthy if it obtains the adherence of a fourth MS. In our ordinary judgments on testimony, a statement in which two or three credible witnesses agree may not be supposed to

impossible reading and the Western text a quite suitable one, he will not admit it as conceivable that it may have reached the West by an independent tradition. It could have been only a lucky guess, and he holds himself quite free to make a better guess of his own if he can.

A good example is Acts xii. 25, where in the "neutral" text the historian is made to say that Barnabas and Saul returned *to* Jerusalem. But the previous history had brought them to Jerusalem, and gives no hint that they had ever left it. Indeed, as the verse goes on to state that they took with them John whose surname was Mark, who is immediately after found as their travelling companion, there can be no reasonable doubt that it was *from* Jerusalem that they took him. Now the narrative immediately before this verse had told of events

gain much from being corroborated by another witness less accurate or faithful; but at least it is not thought to lose anything in credibility from this accession of testimony. But WH's experience is that the stream of testimony suffers perceptible deterioration when less pure elements are allowed to mingle with it.

which had occurred in Cæsarea, and it was a very intelligible mistake, if, at the point where the narrative returns from Cæsarea to Jerusalem, an early scribe should write returned *to* instead of *from* Jerusalem. And such was the form in the parent of the "neutral" text. But if we want to know whether or not this was the form of the original, we have only to consult another line of transmission. On the question whether the sense of the original was "from" or "to" a Latin MS. gives as distinct an answer as a Greek one; and the Latin MSS. are unanimous in favour of "from." But Hort urges that of the authorities which reject the reading $\epsilon\iota\varsigma$, some have $\acute{\alpha}\pi\acute{o}$ and some have $\epsilon\grave{\xi}$. Neither of these was likely to be changed into the other, and still less into $\epsilon\iota\varsigma$; therefore both must be rejected as conjectural attempts to remove a difficulty; and so he feels that there is room for a conjectural emendation of his own, which has no manuscript support, but which has at least the merit of retaining the $\epsilon\iota\varsigma$ of his favourite authority. But why must $\acute{\alpha}\pi\acute{o}$ and $\epsilon\grave{\xi}$ be *both* conjectural alterations? May

not one have been a conjectural alteration, and the other the true reading?

This is not an isolated case. Hort (p. 132) derives one of his proofs of the priority of his neutral text from the cases of simultaneous aberrations from it of the Western and Alexandrian texts, especially "when they severally exhibit independent modes of easing an apparent difficulty in the text antecedent to both." Here he evidently assumes that the discordance between the two modes of easing the difficulty shows that they are both mere guesses, and can claim no authority. But this view is altogether suggested by his nomenclature. If he had called his neutral and Alexandrian texts "early and later Alexandrian," it would at once suggest itself that the solution presented by the latter might probably have only been a critical conjectural emendation of what was felt to be a faulty reading in the current text of the region; but that the Western solution coming through a quite independent line of ancestry was very likely to be the true one. Hort's judgment is quite the reverse: if he is

forced to choose between a Western reading and a later Alexandrian, he prefers the latter, seeming to make it a matter of conscience not to admit anything to appear on his pages on merely Western authority. Thus in the present case he reports that *ἀπό* appears to be the Western reading, and therefore he does not give it admission even to his margin where *ἐξ* is found for those who are not satisfied with *εἰς*. To me the Western readings seem to be so clearly one stage higher in seniority to the later Alexandrian that it is unjust to place them on a level. In particular it seems to me nothing less than an outrage to print the shorter conclusion of St. Mark on the same page with the received longer conclusion; for I cannot believe that the editors would have given a place in their New Testament to a passage having such manifest marks of spuriousness, if it were not intended by this undeserved honour conferred on the former to degrade the latter to the same level. So I have heard of a village apothecary and village attorney being astonished at receiving dinner invitations from

a neighbouring haughty peeress ; the explanation being that her lord had insisted on her inviting a certain small squire, and this was her way of preventing him from being too much elated by the compliment.

However, there is nothing that Hort fights more against than the idea that his neutral text can properly be called "Alexandrian." He eagerly catches at the notion that B, its principal representative, was written, not at Alexandria, but probably at Rome. The reasons for regarding the text of B as Alexandrian remain the same no matter where this particular MS. chanced to be copied. However, the theory that it was written at Rome has not commanded assent, and we can assign a different locality for its origin with a degree of probability which is astonishingly high, when we consider the difficulty of the problem. On palæographical grounds both B and **N** have been adjudged to belong to the fourth century, and probably not to be later than the middle of it. Now Tischendorf discovered (for no weaker word is suitable, his demonstration having convinced Hort him-

self) that these two MSS. were contemporary, and issued from the same workshop. Three sheets of **N**, holding places in distant quires, would seem to have been cancelled and replaced by others in a different hand; and this hand has been recognized as that of the scribe who wrote the New Testament in B. And yet **N** and B are not copied either one from the other or both from the same MS. It is plain then that they came from a great workshop where the copying of New Testament MSS. was going on, and whose resources were such that two different scribes could each be given a different MS. to copy. It does not appear to have been thought necessary that the two archetypes should be compared¹ or any attempt made to harmonize their disagreements, whether it was that such punctilious accuracy was not cared

¹ One case of such comparison would seem to have been the concluding verses of St. Mark, which apparently existed in the archetype of **N**, and were struck out by the corrector of the transcript; very probably by the authority of Eusebius himself, whose opinion is known to have been adverse to these verses.

for,¹ or else because the limited time allowed for the performance of the work did not permit too minute care. Now, at the very time to which these two MSS. have been referred, Cæsarea was a great centre for the multiplication of MSS., and Constantine, about 332, had given an order for fifty complete copies of the Scriptures by skilful calligraphers for the use of the Churches in his new capital. Whether all these copies were made at Cæsarea or not, it must certainly at the time have been

¹ Origen in some cases takes notice of variations of readings, and expresses his preferences; yet in other cases he is found quoting the same texts differently on different occasions; from which it seems may be inferred that he had not been solicitous to bring the MSS. which he used to uniformity. This will be less surprising if we bear in mind how very modern is the minute care that is now deemed to be necessary. It is enough to quote Scrivener's verdict on the performances of his predecessors in the work of collation, all of them men to whom sacred literature is under great obligations.

Of Archbishop Ussher's collation of Codex D he says: "I am grieved that truth compels me to say that I never examined a performance more inaccurate than this. Besides numberless omissions, manifest typographical errors, a looseness and carelessness of citation which is

a place of busy book-manufacture, and it is extremely probable that B and N were written there at that time. It presents no difficulty that the contents of neither correspond with the catalogue given by Eusebius in his Church History, for it may be supposed that the Cæsarean scribes followed their respective archetypes. Now the Cæsarean library owed its chief treasures to Origen, so it is not surprising that the Cæsarean Bibles should contain an Alexandrian text; and it was to be

remarkable, and almost constant inability to distinguish the first from the later hands, its actual misstatements are so many that I have accumulated a catalogue of 228, with which it is needless to trouble the reader."

Of Mill he says: "Largeness of view, critical sagacity, wide and lifelong research comprehend Mill's claim on our gratitude for his great services to textual criticism. Those who award him the humble praise of an accurate collator can have used his edition of the Greek Testament but little."

Of Wetstein: "Too many of his readings are marvelously untrue."

Of Bentley: "The readings he gives for Codex D are few and vague and inexact enough, but no one who has examined his collation of the Codex Augiensis will expect much in this way from our great Aristarchus."

expected that this text, supported by the authority of Eusebius, who was a great admirer of Origen, and afterwards by that of Jerome, should greatly influence subsequent copies. In fact, the only thing to be wondered at is that this did not become the unique type of New Testament text.

CHAPTER III.

THE SYRIAN "TEXTUS RECEPTUS."

IT remains to examine the account which WH give of the origin of the text that actually obtained ascendance before the end of the fourth century. I can readily assent to WH's dictum that we cannot pronounce on the goodness of documents without knowing their history; but the difficulty is that in scarcely any case is there any record of the history, which therefore has to be obtained by scientific divination. That is a method of writing history, of which Renan gives an ingenious defence in his *Life of Christ*; yet most people prefer to trust the documents even when they contradict his divinations. On the other hand, when there are no documentary records, we are obliged to trust to scientific inference, which, if it does not attempt to go too much into details, can yield

results deserving of great confidence. A geologist can feel perfectly sure that once on a time there were volcanoes in one district, glaciers in another, though there is no historic record of these facts. On this account, when experts such as Westcott and Hort report that what they call the Syrian text, which may be described as the *Textus Receptus* of the MSS., gives them an impression of lateness, I do not refuse to accept their decision, even though the proofs which they offer seem to me to come short of demonstration.

4 The proof on which they seem most to rely is the existence of "conflations" in the Syrian text. It is an obvious principle that if a MS. is known to be a copy of an existing MS., the testimony of the copy adds nothing to that of the original, and, in making our list of witnesses, the two count only as one. There are extremely few instances in which we have such clear evidence of the parentage of MSS. as to realize the case supposed ; and, indeed, we have reason to think that in most cases the parentage of MSS. is not so simple. The scribe may

have had two MSS. before him ; or he may have used a MS. in the margin of which had been written readings derived from another authority, all of which he may have incorporated in his text. If the margin had suggested a word as an alternative for one in the text, the scribe may have faithfully copied both ; and hence arises what Hort calls a conflation. To take one of his own examples : Stephen is described in Acts vi. 8 as *πλήρης πίστεως* according to some authorities, as *πλήρης χάριτος* according to others ; one MS. combines the two, *πλήρης χάριτος καὶ πίστεως*.

Now Hort's verdict on what he calls the Syrian text is that it exhibits signs of modernness, both in other ways, and especially by repeated conflations of readings given in the earlier forms of text. Of these conflations he gives in his Introduction eight examples, of which it will be enough here to quote the simplest. The last verse of St. Luke's Gospel, "They were continually in the Temple, praising and blessing God," expresses the reading of the vast majority of extant MSS. ; but the Vatican

and Sinaitic read *εὐλογοῦντες τὸν Θεόν*, while Codex D and the early Latin versions have *αἰνοῦντες τὸν Θεόν*. Hence Hort infers that the current reading is but a conflation of two earlier readings; and, in choosing between them, he gives the preference to the authority \aleph B, which on other occasions he has found the most trustworthy, and edits *εὐλογοῦντες*. But it is evident that another explanation may be given of these so-called conflations. It may be held that the fullest form was the original; and that the two simpler took their origin from one transcriber having omitted one of the participles, and a different transcriber having left out the other. Canon Cook elaborately discussed Hort's eight cases, contending that in every one of them the conflation hypothesis gives the less probable account of the facts. In each of these cases I did not myself follow Hort altogether without misgivings. For instance, before we attribute "mixture" to the Syrian text, we ought to have evidence that the supposed reviser had the materials to mix; and, in the case last cited, there is no evidence that the reading

αἰνοῦντες by itself was ever known in the East, the witnesses to it all being Latin. But no doubt it may be said that these Western witnesses bear testimony to the fact that the original contained but one participle; and, if so, we need not hesitate to accept the Greek testimony as to what the participle was. But, as I have already said, I do not care to examine minutely into those eight examples. It would be necessary to do so if Hort's case rested on these examples only; but I am quite ready to believe that these were but specimens of a much larger number on which Hort's induction was founded.

One general remark, however, must be made. It is a maxim in criticism that, when we have to choose between different readings, the true solution is that which will account for all the variations. In the case of a supposed conflation, if the full reading be the right one, the two defective forms are at once explained. Thus, if *αἰνοῦντες καὶ εὐλογοῦντες* be right, the separate readings *εὐλογοῦντες* and *αἰνοῦντες* are explained as due to accidental omissions by different

hand ab
Latin
cf 45
56

4

scribes. But if εὐλογοῦντες be right, how came any one to write αἰνοῦντες? Until some satisfactory account of this has been given, the problem is only half solved. One can scarcely be contented with Hort's explanation—*viz.* that the cause was mere perversity on the part of Western scribes, who were apt to think one word as good as another, provided the sense was not affected.

I know too little to venture to contest Hort's statement (p. 106) that there are no cases of "neutral" readings apparently conflated from Western and Syrian; though I cannot help thinking that if there was any case of the kind it would be hard to get Hort to admit that it *was* a conflation. There is one notable case which I think he would have set down as one of conflation if the "neutral" and "Syrian" readings had been interchanged. I refer to the case of the "one thing needful"—ἐνὸς δέ ἐστιν χρεία (Luke x. 42). So it is read in the Syrian text, but the saying is one which has not always been given the high spiritual meaning which so many preachers have found in it.

It has been understood to mean that, whereas Martha had troubled herself in the preparation of many dishes, one was all that was really necessary. It would seem that Western scribes thought this limitation somewhat too ascetic, and read *ὀλίγων δέ ἐστιν χρεία*. B combines the two readings, *ὀλίγων δέ ἐστιν χρεία ἢ ἐνός*, which, I suppose, one may translate, "a few dishes are all that is necessary, or perhaps even one would do." If this be the original reading of the autograph, I think this case is an exception to Hort's canon (p. 27): "In literature of high quality it is, as a rule, improbable that a change made by transcribers should improve an author's sense."

Admitting, however, as I am willing to do, the posteriority of the Syrian text, we are still only at the beginning of Hort's account of its origin. Of those who have attempted to form a geological history of the world, there have been two schools: those who suppose our earth to have arrived at its present state by a process of silent and gradual change, and those who have imagined a series of convulsions

or abrupt transitions. WH, in their history of the text, belong to the latter school. They hold that the form of text which we find predominant in the East at the close of the fourth century took its origin from an authoritative revision made about the middle of that century (say A.D. 350) by some leading Antiochian critic.

Although I accepted this ruling on Hort's authority, I felt some difficulties, which I think it well to state at length, because they may be felt by others; and therefore I use the opportunity for stating also some considerations which a good deal mitigate the force of these difficulties.

I felt it as a difficulty that history has preserved no record of this reviser's name, nor indeed of the fact that this revision took place at all. There is no difficulty in conceiving that one form of text may have obtained predominance through a process of silent and gradual change. For instance, it would not be easy to name the person through whose influence the great laxity in English spelling which existed

three hundred years ago passed into the practical uniformity which prevails now. However, that the revision makes on such a critic as Hort the impression that it is all the work of a single hand is a fact entitled to great weight, which I felt that I could not lightly set aside. Still, I could not but think that if so it could not have been so late as 350. In the first place, the time is too short to account for the ascendance it obtained in the last quarter of the fourth century. If this ascendance was obtained through the interference of ecclesiastical or state authority, the period is in the full light of history, and it seemed surprising we should hear nothing of any such interference. If authority could succeed in establishing the views of a distinguished critic, it must have done so when Eusebius enjoyed the favour of Constantine, and we should not have expected the preference of Eusebius for the Alexandrian form of text to have had so short-lived an influence. Surely the critic who made such a revolution in received opinion must have been a man of mark whose name and work would not be likely to be

immediately forgotten. In my opinion, far the most probable explanation we can give why the text of B did not become the *Textus Receptus* is that the form which actually did gain predominance had obtained ascendance in Antioch so early in the fourth century and was then so widely circulated that the Alexandrian form never superseded it. And this view is not inconsistent with WH's final conclusions.

In fact, my quarrel with Hort is seldom because I am unwilling to accept his hypotheses as probable: I only rebel when he puts them on the level of ascertained facts. For example (p. 163), he speaks of the Syrian revision as a *vera causa* as opposed to a hypothetical possibility. I cannot count it as more than a probable hypothesis, and it is a great deduction from the probability of a hypothesis if it requires, like the Ptolemaic theory of old, to be constantly shored up by new hypotheses. For example, the first great difficulty in the way of the acceptance of the doctrine of the modernness of that which had become the *Textus Receptus* so early as Chrysostom's time was that the

Syriac Peshitto, which had been believed to be as early as the second century, agreed in the main far more with the Syrian than with the Alexandrian type of text. The solution then was: "So much the worse for the Peshitto; it cannot be so old as it has been imagined to be; there must have been an earlier form of the Syriac text." Now it is a real test of the goodness of a scientific theory if it enables one to make predictions, and this scientific prediction was verified by Cureton's discovery of a Syriac version of a pre-Syrian type. Then it had to be owned that, on account of the dearth of very early Syriac literature, when a question arises as to the exact form of the text of Syriac translations, very few of our proofs go behind the fourth century. And therefore the Peshitto cannot be relied on as sufficient proof of the antiquity of the "Syrian" form of text. On the other hand, proof of the existence of another early form of Syriac version does not disprove the antiquity of the Peshitto, nor does it even prove that any one Syriac version is entitled to be called *the* old Syriac Version; for it may

be that from an early date versions differed, according to differences in the Greek copies which different teachers used. In particular it is very likely that, as I already remarked, the Greek MSS. which Tatian brought with him from Rome were of the Western type. However, Hort finds it necessary to add to his hypothesis of an authoritative revision of the Greek text about 350, the hypothesis of a corresponding revision of the Syriac text in which the "Syriaca Vetus" was supplanted by the Peshitto.

But it was found impossible to stop here. The Peshitto does not follow the Greek text of the latter half of the fourth century, but stands intermediate between that and the pre-Syrian texts. Hence we cannot assign to it as late a date as 350; and therefore Hort is obliged to postulate two authoritative revisions of the Greek text, the first of which might possibly have been as early as A.D. 250. The hypothesis of three authoritative revisions, two of the Greek text, and one of the Syriac, not one of which has left any trace in history, has become

so complicated that it seems simpler to fall back on the belief that whatever changes took place in the text were silent and gradual. A change that is considered an improvement is rapidly followed, and though probably the most important changes were first made by individuals of some repute as critics, yet we need not wonder if we are not more able to give an account of their history than we are to name the originators of the changing fashions of dress, each of which, by whomsoever started, rapidly becomes general.

I have stated at length the difficulties I felt in accepting Hort's hypothesis of a formal revision ; but it is only fair that I should add that on consideration I do not think it incredible that some such thing should take place without leaving any mark on history. We must bear in mind that the most important copies of the New Testament books were not made for the closet use of students, but for the purpose of being read publicly with the official sanction of the Church. And in any place copies made for private use would naturally conform to the text with which public use had familiarized the

ears of the people. No doubt it was the general use of King James's translation in public church-reading which caused it before very long to supersede all other versions in the private use of the English people. Now in each Church the bishop had complete authority over the church-reading; and unless in his innovations he introduced something that offensively grated on the ears of the people, even considerable changes might only excite a passing remark, and in a little time would become as familiar to the congregation as the old readings. The changes which Hort ascribes to his "Syrian Revision" tending all in the direction of clearness and fulness, so far from being likely to excite repulsion, would be apt to be cheerfully adopted. If then a scholar who possessed the confidence of his bishop produced a revised text, it needed only that it should obtain the sanction of the bishop, when it would come into ecclesiastical use and become in that place the authorized text; and yet without any public mention of the name of the reviser.¹ If this took place in

¹ It is difficult, however, to make any *great* change in a

a great see such as Antioch, the example of the leading bishop would soon be followed, and the text of the chief city would become the text of the district. And changes in the Greek text would tend to produce corresponding changes in versions. For if the bishop of a Church which used a Latin or a Syriac translation came to know that the Gospel as read in his Church did not correspond with that which was then believed to be the Greek original, he would be constantly pressed with the desire to make the use of his Church agree with the current Greek use of his time.

The occurrence of a silent change in the text publicly read in church is not a mere possibility, for such a change actually occurred. Whole books which at one time were admitted into

long-established version. The attempt made in 508 by Bishop Philoxenus to introduce a Syriac version more in accordance with the then approved Greek than the Peshitto failed to supersede the latter version. So also I see no sign that the Revised English New Testament is likely to supersede the Authorized Version, though I dare say it might have succeeded if the changes had been much fewer and more moderate.

the public reading of different churches gradually dropped out of ecclesiastical use, doubtless owing to the rulings of different bishops anxious to maintain the exclusive authority of our canonical books; but of the details of the process there is no historic record. Jerome states (and we need not doubt his evidence) that the Church in earlier times had used the LXX. Version of the Book of Daniel, but in his time employed Theodotion's translation; but professes himself unable to tell how or when the change occurred.¹ I therefore think that Hort's hypothesis of a formal revision cannot be summarily rejected on the ground that there is no historic record of such an event; but the hypothesis becomes much more

¹ As far as Carthage is concerned, we can specify within narrow limits the time when the change occurred, and can even make a fair guess at the author. Tertullian, in his quotations from Daniel, uses the LXX. Version; but, though not more than twenty-five years later, Cyprian employs Theodotion's. Cyprian excelled his immediate predecessors in culture, in energy, in independence of judgment, and none of them was so likely as he to have authorized the change. One who passes rapidly, as Cyprian did, from a lay position to the episcopate is the most likely to

credible if it is simplified, and limited to one such revision about 250. And if we imagine that a revision then took place with the object of arbitrating between the competing claims of an Alexandrian and a Western text, we are not entitled to assume that the former had been in possession in Syria and that the latter was the intruder. It seems to me more likely that the case was the other way. And when we speak confidently of a revision in 250, we are bound to remember how very scanty is our information as to readings before that date, unless we adopt WH's other hypothesis that this antiquity can be ascribed to readings in which B and **Σ** agree.

It is just possible that there may be another

step out of the beaten track and to take a new departure, whether in doctrine or in ritual. Now we find Cyprian in his controversy with Stephen in active communication with Eastern bishops; and probably this intercourse did not then take place for the first time. So that we can easily imagine his deacon Rogatianus, or some previous emissary, bringing back with him from the East a copy of Theodotion's version, with the report that it was there in ecclesiastical use, and was regarded as more faithful to the Hebrew original.

trace of Cyprian's intercourse with the East. The Codex Bobbiensis (known in critical editions as *k*) contains fragments of Latin Gospels which, as far as they go, agree with the quotations of Cyprian. But they contain also the later Alexandrian conclusion of St. Mark, which is found in no other early Latin MS. If a MS. came from the East to Carthage differing in some respects from the text approved at Rome, Cyprian was likely at the time to have given the preference to the non-Roman authority. If the course of public affairs had been peaceful, there might have ensued a permanent divergence in several respects between Roman and African use. But persecution enabled the two rival bishops by their glorious deaths to win equal veneration from the whole Church, and their points of difference fell into the background. Thus while *k* is valuable as making it very probable that the later Alexandrian supplement had been added before the year A.D. 250, its solitary testimony cannot be relied on as proving anything as to earlier African usage.

Supposing now we agree to accept as established the fact of an authoritative revision of the Greek text in the third or fourth century. I hesitate a good deal about WH's next step. They argue, that if the Syrian text was formed out of earlier texts, we have no reason to suppose that the editor was a person of such skill and judgment that we are bound to acquiesce in his decisions. On the contrary, there is good reason to think that his tastes and preferences were different from ours. He liked a text that was full and smooth ; and he did not recognize in ruggedness a mark of antiquity and originality.¹

¹ How much the taste of one age differs from that of another we have an excellent example in an apparently conflate text—the exhortation at the beginning of the Daily Service in the English Book of Common Prayer. There we are told that we ought to “acknowledge and confess” our manifold “sins and wickedness,” that we ought not to “dissemble and cloke them” when we “assemble and meet together” to ask of Almighty God the things that are “requisite and necessary,” etc. We have no reason to think that the author put together two older forms, one exhorting people to confess their sins and another to acknowledge their wickedness ; but simply that his taste regarded the fuller form as the more impressive.

Shall we not then do best if we disregard his decisions altogether; if we take the texts that lay before him and choose between them for ourselves?

Now, if we disregard his decisions we must disregard the authority of the great bulk of our existing MSS., almost all of which do nothing but reproduce for us the Syrian revised text. And this is what we are recommended to do—*viz.* to attend only to pre-Syrian authorities. If we have to choose between two readings having such attestation, the fact that one of them was preferred by the Syrian reviser ought not to influence our judgment in the least, though the result of his preference may be that the one reading is now that of only a couple of MSS., while the other is read in every other extant MS.

But before we dismiss the rulings of the Syrian reviser as absolutely undeserving of consideration, it ought not to be left out of sight that he had one important advantage over us in his better knowledge of the current texts of the fourth century. We are not entitled to assume that his decisions must always be

ascribed to the bad taste that led him to prefer a fuller and smoother reading to the concise and rugged one in which our better judgment recognizes marks of antiquity. It may be that he merely followed the current reading of the MSS. of his time ; and though we can now with great equanimity reject the text of the great bulk of our MSS., and follow the two oldest, yet we might be less confident in our decision, if we knew that at the beginning of the fourth century there was an equal preponderance of authorities against us.

CHAPTER IV.

THE OMISSIONS OF THE WESTERN TEXT.

THE multitude of authorities for the New Testament text is so enormous, and the difficulty of adjudging between rival claims so great, that we ought to be grateful to WH for simplifying so complicated a problem by at once striking away all post-Syrian authorities ; that is to say, they turn out of court all but a few of the witnesses in attendance. We are thus left to choose between the Alexandrian and the Western witnesses. But the latter are next set aside as unworthy of credit, on account of their too great licentiousness and their indifference to verbal accuracy. With one exception, to be presently mentioned, Hort never follows merely Western authority, so that his may be pronounced to be a thoroughly Protestant New Testament, the fact that a

reading is Roman being regarded as enough to condemn it. Yet the Gospel came earlier to Rome than to Alexandria; and Christians necessarily flocking to Rome, as Irenæus remarks, from all parts of the world, must have brought with them MSS. from all quarters. Some of these no doubt would be good and some bad; so that the choice between them might be expected to be difficult: but it does not seem rational to cast all aside *en masse* as absolutely unworthy of examination. I had thought of comparing this successive elimination of untrustworthy witnesses to the process by which Gideon weeded his army of the soldiers on whom he could not rely; but even Gideon's reduced army is too large to represent the force on which WH depend. I ought rather to have thought of the victory won by Jonathan and his armour-bearer; for the majority of the Alexandrian witnesses are not thought worthy of more than a local name; and what Hort really trusts is B with the sometimes doubtful assistance of **Σ**.

Hort's limitation of admissible witnesses to

a single line of testimony has led to two consequences, with neither of which I am satisfied. The first is that he regards conjectural emendations with less disfavour than cautious critics of late have done. Of course the field open for conjecture is the wider the more scanty the MS. evidence. In the case of a classical author, whose text may ultimately depend on the testimony of a single witness, a licence of conjecture is quite legitimate, which is not justifiable in the case of the New Testament, of which we have so many MSS. that it is scarcely likely that the true reading should not have been preserved in some one of them. As new MSS. are discovered our confidence in the most plausible conjecture is abated. When the text of Clement's Epistle rested on but one MS., I thought Wordsworth's emendation (c. 6) *νεάνιδες παιδίσκαι* instead of *δαναίδες καὶ δῆρκαί* almost certainly right. I have not the same confidence now that the since discovered MS. evidence gives the conjecture no support. Evidently the depreciation of all authorities except the early Alexandrian leads to willing-

"neutral."

ness to accept conjecture as some relief for the voluntary poverty to which we have reduced ourselves.

The second consequence of Hort's confidence in his system is his belief that it is liable to little modification from subsequent discovery of MSS. or from increased knowledge by careful collation of existing MSS. His "siege has been made," and he thinks it a work of little necessity to add to his materials. In fact, though Hort bestowed enormous labour on the formation of his system, he has enabled his disciples to set up as critical editors with as small an apparatus as Burgon himself might have found necessary. Burgon might have given the instruction: "Follow the *Textus Receptus*, unless you come across something very clearly wrong." Hort does not give the instruction, "In the Gospels always follow B," any more than Cato the elder explicitly laid down that agriculture, and especially pasturage, was the only way in which a gentleman could honourably make an income. But Hort's rules very much remind me of Cato's "A quo cum

quæreretur quid maxime in re familiari expediret respondit, Bene pascere. Quid secundum? Satis bene pascere. Quid tertium? Male pascere. Quid quartum? Arare. Et cum ille qui quæreret dixisset, Quid fœnerari? tum Cato, Quid hominem, inquit, occidere.”¹

So Hort, if consulted what authority should be followed, might answer, Follow B **N**; accept their readings as true, unless there is strong internal evidence to the contrary, and never think it safe to reject them absolutely. But suppose B has not the support of **N**? Still follow B, if it has the support of any other MS. But suppose B stands alone? Unless it is clearly a clerical error, it is not safe to reject B. But suppose B is defective? Then follow **N**. What about adopting the Western reading? What about killing a man?

If more new MS. evidence should come to light, there is no reason why it should be treated with more respect than that which has been already rejected. The first question

¹ Cic., *De Off.*, ii. 88.

would be, Is it post-Syrian? In that case it might be treated with the same disregard as the great bulk of our existing MSS. which are of that class. Much the same might be said if its text were Western; and *that* even if the MS. were undoubtedly as old as the second century. If by a rare chance its text was of the same type as \aleph B, then indeed it would be valuable, and would give additional strength to the conclusions already arrived at; but no possible find could affect these main conclusions, though it might be interesting to note some sporadic varieties of reading, and though its testimony might be accepted as throwing light on a few cases where the present evidence seems nearly balanced.

There is just one case where it would really deserve to be listened to with deference, and that is if it omitted anything found in all our present authorities. Hort himself, in criticising Tischendorf's Greek Testament, had censured him, because "he makes the worst, or at best very bad, evidence if supported by a canon of probability outweigh the best evidence standing

alone."¹ He himself had insisted that it is exceedingly precarious to ignore the relative antecedent credibility of witnesses, and trust exclusively to our own inward power of singling out the true readings from their counterfeits. Consequently he lays down as a canon that *knowledge of documents should precede final judgments upon readings*. Accordingly he examines the documents with the result that among readings whose attestation is ancient, he attributes the very highest value to the combination **N B**, and the very lowest to the Western readings. But when it is a question about omission, if a small handful of Western MSS. omit words which are found not only in **N B**, but in all other authorities, he decides in favour of omission.

The inconsistency is very marked in the last verses of St. Luke's Gospel. The very same authorities which omit *καὶ ἀνεφέρετο εἰς τὸν οὐρανόν* in ver. 51, read *αἰνοῦντες* instead of *εὐλογοῦντες* in ver. 53. Tischendorf thought that he could

¹ *Journal of Classical and Sacred Philology*, iv. 207.

not consistently follow them in the one case and forsake them in the other. Consequently, as he decided on omitting the clause in ver. 51, he thought himself bound to edit *αἰνοῦντες* in ver. 53. WH, acting on their ordinary principles, reject the Western *αἰνοῦντες*; but in a question of omission they follow the authorities they reject in ver. 53, and pronounce the suppression of the clause in ver. 51, as well as of another in ver. 52, to be a case, not of omission, but of non-interpolation.

Hort's reason for always in similar cases preferring the shorter reading is founded on a canon of probability. Transcribers, he holds, were much more likely to add to the text than to omit, their universal tendency being to make their text as full as possible; for no transcriber would willingly omit any genuine word of the Gospel, though, through fear of thus mutilating the divine record, he might include in his copy some clauses on too slender authority. It must be accepted as a fact that in the early centuries the Gospels as read in one place contained some clauses not found in

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the Gospels as read in another. Whether any explanation can be given of this may be discussed later on. At present I will only say that I believe it to be far too extreme a rule to lay down that in the admission of a verse into the New Testament text a single black bean shall exclude. As to the dictum that scribes were more likely to add than to omit, it is true when the work of the scribe was editorial, but not so when it was merely mechanical. A compositor at a printing office might easily drop out a word or line of his copy, but would never dream of inserting one of his own. It may be presumed that the production of the very earliest copies of the Gospels was merely mechanical, and that critical comparison of different copies and additions to them belong to a somewhat later stage.

However this may be, I feel no doubt with regard to the example I have quoted that WH are wrong in treating the clause *ἀνεφέρετο εἰς τὸν οὐρανόν* as an interpolation, and that the word "omission" is properly applied to the

leaving out of words clearly necessary to the context. For if we leave them out, we find that St. Luke closes his Gospel by relating an interview, at the end of which our Lord, who had led His disciples out as far as Bethany, parted from them, whereupon they returned to Jerusalem with great joy. We ask what was the cause of their joy; were the disciples glad that their Lord had left them? And it would seem that they took His departure as final, for thereafter they were continually in the Temple blessing God. Again, we are told that He parted from them ἐν τῷ εὐλογεῖν, in the act of blessing them. This is pictorially quite intelligible if the parting was by way of an ascension, but otherwise it is difficult to conceive in what way a departure in the act of blessing could be accomplished.

The violence of Burgon's attack on the rejecters of the conclusion of St. Mark's Gospel seems somewhat to have disturbed Hort's calmness of judgment, and to have made him keensighted to watch and close every possible door against the admission of the disputed verses. In

this case he takes occasion to profess his belief not only that the story of the Ascension was no part of St. Mark's Gospel, but that it ought not to find a place in any Gospel. He says that this interpolation in St. Luke was inserted from an assumption that a separation from the disciples at the close of the Gospel must be the Ascension; but that this apparently did not lie within the proper scope of the Gospels as seen in their genuine texts. But this does not appear to have been the opinion of St. Luke himself, who, in the beginning of the Acts, states that the subject of his former treatise had been the things which Jesus began to do and teach up to the day of His Ascension, *ἄχρι ἧς ἡμέρας ἀνελήμφθη.*

Now it is remarkable that the same Western authorities which omit the clause in the Gospel vary much from the Received Text in the opening of the Acts. In fact, Blass in his attempt to restore the Western text of the Acts leaves out this reference to the Ascension, and we have instead a reference to a parting charge of our Lord to His Apostles, such as

we find at the conclusion of St. Matthew's Gospel, and of St. Mark's according to the fuller text. Speculation as to the origin of this difference belongs to another branch of the subject, about which a few words must be said later on. Here it will suffice to say that there was an obvious reason why a reviser might be tempted to get rid of the mention of the Ascension at the conclusion of St. Luke; for the impression which the Gospel gives a reader is that the Ascension took place on the same day as the Resurrection, whereas the Acts, written probably after the Evangelist had acquired fuller knowledge by intercourse with those who had companied with our Lord, places an interval of forty days between them.¹ In any case it ought to be seen that the discussion of the true reading of

¹ This interval was further extended to a year and a half by some of the Gnostic sects. Their object evidently was to find space for the communication by our Lord to His Apostles of the secret doctrines of which these sects claimed to be in possession. Although some of these sects arose very early in the second century, the Gospels

the last verses of St. Luke cannot be separated from the discussion of the true reading of the opening verses of the Acts. I am persuaded that critics will be forced to acknowledge that the Gospel as read in the second century in the Church of Rome differed in a few particulars from that read at the same date in Alexandria. Critics may discuss which of these texts is authoritative, or whether both may be so; but I am sure that an arbitrarily created hybrid between the two is wrong; and this is the kind of text more than once exhibited by WH in the closing verses of St. Luke.

There being then a good way of accounting for the dropping out of the clause, what Hort calls intrinsic probability and transcriptional probability agree in approving the reading which has the strongest possible documentary attestation; yet all this avails nothing when

which we still have must by that time have gained such recognition as the only authentic account of the public life of our Lord, that it was felt that it would be difficult to gain acceptance for an addition to the history of the period with which their narrative dealt.

coming into collision with Hort's canon that an addition to the text is always more likely than an omission, or rather, that the former is likely enough, the latter almost inconceivable.¹

If I cannot adopt Hort's decision in this case, the knowledge of the overpowering weight which this principle has with him abates something of my confidence in following his rulings in some other cases. I have already said that we do not get rid of the subjective element if, instead of trusting on our judgment to decide in particular cases which reading is most probably correct, we follow the documents which we have found to be most deserving of confidence. For still everything depends on the correctness of the judgment we form as to which documents are most worthy of confidence ; and after this specimen of Hort's adoption of an omission, in the teeth both of documentary evidence and of considerations of probability,

¹ I do not know whether Hort's rule of always preferring omissions would have led him to prefer to the Greek text of the Gospels Mrs. Lewis's Syriac, which is shorter than any other known text.

a suspicion arises whether it is correct to say that WH exclude certain passages from their text because the best documents omit them, or rather that WH account those documents the best because of their omissions.

The other cases of what Hort designates as Western non-interpolations in the closing chapters of St. Luke's Gospel deserve a fuller discussion than can be given here without lengthening this essay too much. I merely mention a speculation which accounts for signs of compression, at the end both of the third Gospel and of the Acts, by a theory that the papyrus books of the day were made in definite sizes, to which copyists and writers had to conform. Copyists would be most tempted to compression and omission when space was failing them towards the end of their book. If writers had more to say than their book could hold, they made a new book. Thus it has been suggested that the manner in which the Gospel closes indicates that the author contemplated a continuation. So likewise that we may gather from the manner in

which the story of the Acts breaks off, leaving untold much that we should wish to know, that if the author had lived he would have given us a continuation of the history.

CHAPTER V.

THE SYNOPTIC PROBLEM.

THE criticisms I have hitherto made are not offered as more than modest doubts, presented as subjects for inquiry by students who shall have qualified themselves to speak as experts; for I am well aware that one who has no such pretensions is liable to make too rigorous demands for proof, and to reject conclusions which one familiar with the subject can see to be true, although he may not be able to state his reasons for holding them in such a way as to satisfy an outsider of their logical cogency. But I feel myself on firmer ground when I express my opinion that it is an obvious and very serious fault in WH's work that neither of them appears to have taken any interest in the question of the origin of the Synoptic Gospels; that is to say, in

inquiries whether the narratives of the three have any common basis, oral or written. And yet the decisions we come to on such points as these must materially affect our conclusions as to what can properly claim a place in the New Testament text.

In whatever points Burgon differs from Westcott and Hort, they all agree in this, that the critic's business is to go back to the apostolic autographs. The assumption common to them all, for example, is that the first Gospel was originally written by St. Matthew's own hand, or by that of his amanuensis; and therefore that there can be in no case more than one reading which can claim to be original; any changes that have since taken place must be due to transcribers' alterations, conscious or unconscious. Consequently, though there are some of the Western additions to the Alexandrian Gospel text which Hort is willing to recognize as embodiments of ancient and perhaps true tradition, he pronounces them (p. 195) to be quite extraneous to the Gospels "considered as individual writings of individual authors." I shall pre-

sently return to the question whether the value of the Gospels to us is that they contain the individual words of an individual author, of whom apart from his work in some cases we know scarcely anything. For example, do we value the second Gospel because it contains the actual words of St. Mark ; or would its value be less if it could be proved that it was written, not by Mark, but by some other of the Apostles' disciples ? Is it not rather the case that we honour St. Mark because he was the author of a work which was recognized by the Churches which the Apostles founded as containing a specially authentic record of our Lord's early life and was publicly read as such in weekly meetings of Churches ever since the time when eye-witnesses of the events were alive ?

But, postponing this question, I wish to know whether WH claim for their own work that it gives "the individual words of the individual author." I can understand such a claim in the case of St. Paul's Epistles, for then we have every reason to think that there is but a unique original to which we desire to get back. On

this account I consider that one who desires to study New Testament criticism would do well to commence his training by studying the text of St. Paul's Epistles. The third and fourth Gospels have strong traces of the individuality of their authors, and therefore the only doubt as to the uniqueness of their original arises from the possibility that the authors may have published more editions of their work than one. But the second Gospel, and still more the first, give us the impression of being works rather of a compiler than of an original writer. I ask then, Can WH be confident that the first Gospel, as they edit it, presents us with the individual words of St. Matthew? Are we completely to set aside the tradition that the Gospel was originally written in Aramaic? Are the individual words which we try to recover those of St. Matthew himself or of his translator, or perhaps we should rather say of his editor, because our Greek St. Matthew has many marks of not being a mere translation, however true it may be that it was based on an Aramaic original? And were there more editors than one? Is

there any truth in German speculations about a deutero-Matthæus and a trito-Matthæus? Surely it is a very vital question whether the individual words that we seek are those of the Apostle Matthew or of an unknown subsequent editor.

Hort had some misgivings on this point, for (p. 282) he specifies a few passages "in which it is difficult to believe that all the words as they stand have apostolic authority." And in the only passage that I know where Hort exhibits consciousness of the Synoptic problem (Appendix, p. 22) he speaks of "the genuine text of the extant form of Matthew." Surely if the "extant form" of St. Matthew were not necessarily the same as the original St. Matthew—if the work suffered growth or alteration after the time of its first publication, such growth may easily have been antecedent to the authority on which Hort relies. Giving to the common parent of B and \aleph as high antiquity as is claimed for it, still it will be distant by more than a century from the original autographs, and the attempts to

recover the text of MSS. which came to Alexandria in the second century may be but an elaborate locking of the stable door after the horse has been stolen.

If changes took place in the text previous to the origin of the parent of B, they are likely to have been of a different kind from those that occurred later on. One example is worth being discussed at length. Hort regards it as a proof of the modernness of the Syrian text that it is apt to assimilate one Gospel to another. Now it is certainly true that in the case of a transcriber familiar with all our Gospels, even if he did not intentionally alter the text before him, in order, as he imagined, to improve it by bringing it into conformity with another Gospel, it would be a very natural error that he should sometimes unconsciously substitute for the words of the Gospel he was copying, better remembered words from the parallel passage of another. At the end of the second century, when first we have clear external evidence as to the circulation of our Gospels, all our four are found to be known and venerated all over

the Christian world. But in the earliest days of our religion we may well believe that each of the Synoptic Gospels was written for men who had no other ; that each had at first but a local circulation, written in a little papyrus book by itself. In those days the transcriber of a single Gospel would be under no temptation to harmonize it into conformity with another ; but, on the contrary, having no acquaintance with the story as told elsewhere, he might, by introducing errors of his own, cause a dissimilarity which had not previously existed. Thus it might happen that if a story had been originally told in identical words in two Gospels, these might in the process of transcription come to vary, and later transcribers who reduced them to uniformity may have only corrected an error of previous copyists.

Thus our decision on the Synoptic question must affect our decisions on textual criticism. If the same event is related by two Evangelists, then if we regard them as quite independent writers a diversity in their narratives is to be expected, and a complete identity would provoke

suspicion that the text of one or other had been tampered with. But if we believe that the two drew their story from a common source, then identity of narrative would be perfectly natural ; and it is diversity that raises doubt whether the text has been accurately preserved.

When Hort speaks of the tendency of the Syrian text to assimilation of the Gospels, he probably had one notable case specially in his mind—the story of the rich young man (Matt. xix. 16 ; Mark x. 17 ; Luke xviii. 18). According to the Received Text, all the Gospels tell the story the same way : the young man addresses our Lord as “Good Master,” and receives the reply, “Why callest thou Me good ? there is none good but One, that is, God.” But according to **Σ** B the question, as the story is told by St. Matthew, is, “Master, what good thing shall I do ?” And the answer is, “Why askest thou Me concerning ‘the good’ ? there is One that is good.” This reading is confirmed by Origen, who notes that Matthew’s version of the story is different from that of the other Evangelists ; and the reading was consequently

adopted by Eusebius, and after him by Jerome. The reading is also that of D, and we need not here raise the question whether the reason is that D contains an Alexandrian text with separable Western additions,¹ because the early Latin translations agree in the main with this variation.

WH have, therefore, an irresistible case on behalf of the reading which has the unanimous support of the earliest witnesses, coming to us through two independent channels. It is

¹ Perhaps it may not be useless to explain, what Burgon seems to have been unable to understand, why modern critics who own the licentiousness of D still rely on it as an authority ; and in particular why when they reject its additions to the text they value its testimony to omissions. If we imagine that D represents to us a more ancient MS., in the margin of which additions had been written which D has incorporated in its text, then if we could only strike out those additions we should recover the parent text. Tregelles was of opinion that many of these additions are as separable from the text as footnotes are from the text of a modern book. If then, after these additions have been discarded, we find D agreeing with the early Alexandrian MSS. in rejecting some things which are found in the Syrian text, it is inferred that the parent of D had not included them.

true that the story is referred to by several authorities earlier than Origen, who all seem to have known it in a form substantially the same as the Received Text, and not one of whom appears to be acquainted with the reading, *τί με ἐρωτᾷς περὶ τοῦ ἀγαθοῦ*; No doubt it may be said that they were referring to St. Luke or St. Mark, and not to St. Matthew; yet some of these authorities are such as we should have expected to be better acquainted with the first than with the other Gospels; and it is odd that they should not mention the variation of reading. However, since A and the other "Syrian" authorities agree in assimilating St. Matthew's story to that told by the other Synoptics, WH's assertion appears to be well confirmed, that a tendency to such assimilation is a feature of the Syrian revision.

But the case presents a different aspect if we try to go behind the MS. which was the parent of **8** B, and doubtless of other descendants. It is quite plain that it is the same occurrence which is related by all three Synoptics, and the question how the story is related by St. Matthew

is not so important as the question, What were the words which our Lord really spoke? whether did He say, *τί με λέγεις ἀγαθόν*; or *τί με ἐρωτᾷς περὶ τοῦ ἀγαθοῦ*; Now we have here exactly the same question as that ruled by Hort, p. 54, his decision being in perfect agreement with that made by Scrivener.

Suppose we have an original O, transmitted to us through two channels, X and Y. Say that the descendants of X are *a, b, c*, and of Y are *d, e*; then if in some particular case some of the descendants of X, say *c*, separate themselves from their usual allies, *a, b*, and join themselves to *d, e*, we cannot satisfactorily account for the unnatural alliance, except by acknowledging *c* as that which correctly transmits the true reading of X; *a* and *b* having met with corruption from some other quarter. In the present case the story has come through three channels—Matthew, Mark, and Luke; there is no dispute as to the testimony of Mark and Luke, and with them agree the vast majority of the extant MSS. of St. Matthew. If therefore we accept Hort's ruling on p. 54 just cited,

we must set aside the dissentient MSS. of St. Matthew, and accept Luke's version, not only as that which correctly reports our Lord's words, but also as that which must have been found in the original text of St. Matthew.

Reserving the discussion of the latter point, I think we cannot hesitate to pronounce on transcriptional grounds that Luke's version is that which most correctly represents our Lord's words; for there is no reason why, if St. Matthew's version had been the original, it should have been altered into the other form, whereas there was an obvious reason why believers in our Lord's divinity should have been startled by the question, "Why callest thou Me good?" if at least they put on it the first-sight interpretation and regarded it as equivalent to, "You ought not to call Me good."

In respect of intrinsic probability St. Luke's version has manifest superiority over that of the Alexandrian St. Matthew. In the former a quite natural question receives a perfectly appropriate answer. In the latter, not to say that the question "What shall I do?" is more natural

than "What good thing shall I do?" (a form which seems devised to give occasion for the answer about "the good"), it does not seem a fair answer to assume that the young man wanted information *περὶ τοῦ ἀγαθοῦ*, when it was clearly different information that he wanted. Again, the answer that there is only One (*viz.* God) who can properly be called good, is appropriate if intended to exclude the ascription of that attribute to any *person*, but does not convey the meaning that it must not be ascribed to any thing or any action—a doctrine inconsistent with our Lord's own practice (Luke xviii. 15; Matt. vii. 11; Luke xi. 13; Matt. xii. 35; Luke vi. 45). On the whole, as I cannot doubt that the second and third Evangelists give the true report of the incident we are discussing, I find it impossible to believe that the Apostle Matthew gave a different one, and therefore I see only two explanations of the present state of the text.

The first is that suggested by Hort's canon already cited—*viz.* that an alteration was made in the MSS. of St. Matthew, which had been

originally in unison with the other two Synoptics. It evidently must have been made very early—that is to say, while the Gospels were in separate circulation; for whatever reasons there were for making a change in St. Matthew's text would have applied equally to the other Gospels, if he who made the change had been in possession of them. A corruption introduced so early might easily obtain the excessive circulation that this one did.

It must be remarked also that the majority of the witnesses already cited as favouring the altered reading exhibit signs of conflation. In the true text of each of the versions of the story, the word "good" only occurs once in the question; either, "Good Master," answered by, "Why callest thou Me good?" or else, "Master, what good thing?" answered by, "Why askest thou Me concerning goodness?" When the latter version is presented, with also the address "Good Master," we can see that the scribe was then in possession of both forms of the story. Yet the authorities for "Good Master" are so ancient and so numerous—

Origen is one of them—that there was good ground for Burgon's sarcastic comment on the marginal note in the Revised New Testament, "Some ancient authorities read 'Good Master'": "I should like to know how many ancient authorities read anything else." Instead then of saying with WH that the insertion of the word "good" before "Master" was the earliest corruption to be introduced into the text, I should prefer to say that this was the word in the original text which most stubbornly resisted alteration. In any case it does not seem fair to accuse the "Syrian reviser" of having introduced the assimilation of the Gospels. It probably only was that, in choosing between the different forms of text current in his time, he preferred that which made all the Gospels agree in their narrative. So much for the first explanation.

The second explanation is to suppose that our first Gospel is by no means a copy of St. Matthew's autograph, or even a faithful translation of an Aramaic original; but a work which, though probably founded on St.

Matthew's, had received additions or alterations from one or more subsequent editors. According then to this theory, WH may have correctly edited the text presented in the final edition of the first Gospel, which, however, we are not at all bound to suppose agreed in all respects with Matthew's original. This example will show how close is the connection between the criticism of the Gospel text and theories concerning the genesis of the Gospels, and how much in my opinion the work of WH has suffered from their want of interest in the latter inquiry.

We can hardly separate from the discussion of this text in St. Matthew the addition made to chap. xxvii. 49 of the same Gospel: "Another taking a lance pierced His side, and there came out water and blood." Thus this piercing, which according to St. John took place after Jesus was dead, is made to have taken place while He was alive. The evidence for this reading comes short of that for the Alexandrian version of the story of the rich young man, because it has less support from

the early Latin versions; but it has even in a higher degree all the early evidence on which WH place most reliance. Hort then, as in consistency bound, leans strongly to the opinion that the words belong to the "genuine text of the extant form of St. Matthew,"¹ and were early omitted (originally by the Western text) on account of the obvious difficulty. But since he had already stigmatized a few phrases in the Alexandrian text of the concluding chapter of St. Luke as "non-Western interpolations," he thinks it safer to treat this verse also as an early interpolation, which never found admission into the Western text, and so he

¹ This remarkable phrase suggests the question, What is supposed to be the date of the "extant form of St. Matthew"? If we are to insert in our New Testament a verse which we do not believe to have been written by St. Matthew, why contend so vehemently against the concluding verses of the second Gospel because we do not believe them to have been written by St. Mark? Those verses were read as part of St. Mark's Gospel in the second century by Irenæus; and as I believe were so read at the same date in the Church of Rome. Is there evidence that the additions made in the "extant form of St. Matthew" are any older?

includes the verse in his text, but on the same conditions of double brackets as those on which he received the "carried up into heaven" at the end of St. Luke's Gospel.

Hort's phrase "the genuine text of the extant form of St. Matthew" implies, as I have said, a consciousness that he had not succeeded in getting back to the apostolic autograph. I have no doubt that he has succeeded wonderfully well in getting back to the text of the ancient MS. from which **Σ** and B were ultimately derived; and I have little doubt that that MS. must have contained the clause now under consideration. But since it can be asserted with certainty that this clause was not part of the original St. Matthew, and with high probability that it was added by some one already acquainted with the Gospel of St. John,¹ it follows that what WH have

¹ On the authority of an ancient scholiast who speaks of Tatian as an authority for this reading in St. Matthew, Scrivener and Burgon accounted for this intrusion of a passage from one Gospel into another as originating in the use of the Diatessaron, which mixed together the words of different Gospels. When recent discoveries

reproduced does not represent the apostolic autograph, but a text which had undergone alteration, more or less ; and therefore that we have no right, relying on its authority, to refuse to take into consideration what was read in other parts of the Christian world. As for the "Syrian reviser," we have no means of knowing on what MS. evidence he rejected this "non-Western interpolation" and accepted the others at the end of St. Luke ; but there is good reason for thinking that in both cases his judgment was right.¹

I have said enough to illustrate my opinion that no one who ignores the Synoptic problem

seemed to enable us to restore in great measure the text of the Diatessaron, it was found to give no sanction to this explanation. But Burgon held fast to his explanation, and refused to acknowledge the restored Diatessaron, which he always refers to as Pseudo-Tatian. I observe that Miller also uses the phrase "Pseudo-Tatian." I do not know whether it is a necessary inference that he shares Burgon's opinion about the Diatessaron.

¹ As another instance how our judgment about readings is affected by our opinions as to the genesis of the evangelic texts, I may mention that Hort rejects a couple of those so-called non-Western interpolations in St. Luke because he judges them to have been added by some

has a right to be confident in the judgment he forms on the text of a passage in one Gospel without having considered the parallel passage in another. Thus, for example, there is no doubt as to the reading of Luke vii. 35, "Wisdom is justified of all her children"; but in the parallel passage, Matt. xi. 19, WH, on what would be very weak evidence if the combination \aleph B were not regarded as absolutely decisive, read, "Wisdom is justified by her works." The Revised Version follows them, but gives a note, "Many ancient authorities read 'children,'" which tempts one to repeat a sarcastic comment of Burgon's already quoted. Now if we ask which of the two was the phrase actually used by our Lord, the following considerations favour our adoption of Luke's account: (1) that there seems reason to think that it was not the purest form of St. Matthew's

one acquainted with St. John's Gospel. But if I am right in thinking that the author of the fourth Gospel shows acquaintance with the third, coincidences would rather indicate that the third Gospel had assumed its present form before the fourth was written.

Gospel which was found in the copy which was the parent of the Alexandrian; (2) that after St. Paul's Epistles had become the common property of the Churches, the phrase "justified by works" would present a familiar combination which might easily come accidentally from the pen of a transcriber; (3) that the form given by St. Luke is much more difficult of interpretation than that ascribed to St. Matthew. On the other hand, an attempt may be made to account for the variation by seeking to recover the Aramaic word used by our Lord;¹ yet the preservation of the word "all" in one version and its absence in the other can scarcely be explained unless there is acknowledged to have been some conscious manipulation of the Greek.

During the considerable interval between the composition of the Gospels and the earliest date to which we can trace the source of any of our existing copies, there plainly was time enough

¹ Exactly the same confusion has been pointed out by Lightfoot as having taken place in the Syriac translation of Clement (see Lightfoot's *Clement*, i. 138).

for errors of transcription to arise. I fear I am doing an injudicious thing in here mentioning a doubtful speculation of my own on this subject, as it may indispose readers to accept other conclusions about which I have more confidence. But it seems to me a possible thing that it may have been an early transcriber who is responsible for a well-known difference between Mark and the other Evangelists—*viz.* that Mark alone makes the cock which woke the slumbering conscience of Peter crow twice. If we ask what were the words actually spoken by our Lord, “before the cock crow,” or “before the cock crow twice,” we have the other three Evangelists united against St. Mark in favour of the former account; for in this instance John joins his testimony to the Synoptics. St. Mark’s disagreement is the more surprising because Matthew’s account of the Passion closely, and often verbally, follows St. Mark’s, or else that of Mark’s authority; only inserting from time to time matter derived from some other source, but after each insertion taking up St. Mark’s narrative where it had been broken off.

If any one will take the trouble to draw a line in his Bible along those verses of Matt. xxvii. which are common to St. Mark, he cannot help seeing how closely the latter is followed, and how little of it is left out, and he will be at no loss to answer the question whether Mark's account is an abridgment of Matthew's, or Matthew's an expansion of St. Mark's. There is no such very close agreement between these Gospels in other places, and it becomes hard to believe that in this case there was original disagreement between the allied authorities.

But when we examine into the evidence for Mark's singularity we find it breaks down a good deal. Hort has remarked that of the seven principal MSS. of St. Mark which tell the story no two have exactly the same text. It was really Hort's attempt to show that every version of the story except B's has suffered from transcribers' errors which impressed me with the idea that the same thing may be said of B's. Hort points out that Mark's variation is made in four places : (1) our Lord's prediction (xiv. 30), "before the cock crow *twice* thou shalt deny

Me thrice"; (2) after the first denial (ver. 68), "*and the cock crew*"; (3) after the third denial (ver. 72 *a*), "*and the second time the cock crew*"; (4) Peter's recollection of our Lord's prediction (ver. 72 *b*), "*before the cock crow twice.*" No two of the authorities deal in the same way with all these four places; but in the various ways in which they present these particulars, Hort finds that B alone has the note of genuineness as giving the points "tersely but sufficiently." B gives the points (1), (3), (4), but omits (2); yet surely if Hort had not been so prejudiced in favour of B as to find it almost impossible ever to desert it, he would have found some defect in a story which tells only of one cock-crowing, yet describes that one as the second.¹

B has but scanty support in this omission; but if we agree with Hort in accepting its

¹ I cannot always sympathize with the admiration which both Hort and Burgon felt for their favourite authorities. Thus Burgon finds in the Received Text of Luke xviii. 14, *δεδικαιωμένος ἢ γὰρ ἐκείνος*, "an exquisitely idiomatic expression," which tempts one to think that he regarded "idiom" as the English equivalent for *ἄπαξ λεγόμενον*.

testimony that the words "the cock crew" in ver. 68 are spurious, the phenomena are easily explained. If an early scribe had inadvertently written in ver. 68 the words which properly ought to come in ver. 72, then subsequent scribes, finding two cock-crowings related, would be under a strong temptation to make the narrative consistent by inserting the word *δὲς* in our Lord's prediction, and adding *ἐκ δευτέρου* in ver. 72. Yet it is surprising what a strong support of MS. evidence there still is for omitting the *δὲς* in our Lord's prediction. In suspecting that Mark's original narrative only made mention of one cock-crowing, I take no account of the evidence of **N**, which does so tell the story, but as I think rather from harmonistic motives than because of then extant testimony; for the editor of this MS. seems to have been somewhat addicted to improving his text by conjectural emendations.

Perhaps it will not have been altogether useless to state a speculation which probably will find but little acceptance, if it enables us to feel more distinctly that the authorities on

which WH rely are still so far from the original autographs, that the careful ascertaining the verdict of these authorities may be, as I said, but an elaborate locking of the stable door after the horse has been stolen.

CHAPTER VI.

THE PROBLEM OF ACCOUNTING FOR WESTERN VARIATIONS.

IT may doubtless be said that WH do not pretend to have succeeded in recovering the text of the original autographs, but only that they have got as near that text as it is now possible to arrive. Still, if any one were to defend the assertion that the British expedition had arrived at Khartoum by saying, Of course I did not mean that they had actually got to Khartoum, but only that they had got as near it as under present circumstances it is possible to arrive, we should reply, If the expedition has only got as far as Dongola, it would be better to say Dongola and not Khartoum. When WH refuse to give a local name to the readings they prefer, and designate them as neutral, that is to say, as free from corruptions of various kinds,

they are disguising from themselves and from their readers that the question what text has the most early attestation cannot be decisively answered.

As I have already said, the honour we give to our four Gospels ultimately rests on the fact that all over the Christian world they were from the first recognized as the specially trustworthy records of the Saviour's life. But this unanimity of testimony respecting the books regarded as substantial wholes does not continue to prevail when we desire to remove variations of readings; for such variations are found to exist as far as we can trace the history back. Some of our most valuable information about early various readings is got from Origen, and when he tells us what were the readings of the MSS. which he accounted the best and oldest, we may safely infer what was then the approved text in the Church of Alexandria. But quite as early testimony convinces us that readings different in several points were at the same date approved in the West. It would seem then that if we desire a text absolutely

free from ambiguity we desire what God has never been pleased to give His Church ; nor do I see that the ambiguity affects the proof of anything that can be supposed to be necessary to our salvation to know.

But if we desire to solve the literary problem of determining what readings can claim to have belonged to the earliest form of the Gospels, it does not seem that success is likely to be attained if we begin by setting aside half the witnesses. Hort's method of casting aside Western readings as worthless has certainly the advantage of much simplifying the problem ; but it reminds me too strongly of the Irish juryman who, after he had heard counsel on one side, decided that it only perplexed his judgment to listen to what the other side had to say. When we have rejected all the "Syrian" witnesses, that is to say, the overwhelming majority of all the less ancient MSS., and all the Western witnesses, that is to say, a majority of all the ancient ones, we find criticism made very easy. We have but to follow B, and are only embarrassed when that MS. fails us, or in the rare cases where its

readings are clearly inadmissible. But considering how early the Gospel found its way to Rome, and what a part that city played in early Church history, it is strange that its testimony to the earliest form of the Gospel text should be so very worthless.

No doubt it is credible that bad MSS. as well as good found their way to the West. As there were early Alexandrian and later Alexandrian, so there were early Western and later Western; there were Western texts which were Roman, and others which were Western in a wider sense. Thus the task of discrimination may be difficult; but we must not conceive that we have solved a problem because for our convenience we have simplified it. True such simplification may be a first, and a necessary, step to the solution. It is thus that in theoretical mechanics we study the operation of forces, at first putting out of sight such disturbing elements as friction, resistance of the air, etc., which complicate the practical problems with which we have to deal. And doubtless the study of the Alexandrian evidence

by itself is an important simplification of the critical problem, and has led to results of permanent value. But the problem has not been completely solved until we have taken account of the evidence which has been temporarily neglected. When modern explorers set themselves to discover the sources of the Nile, they found after they had traced the river a long way up, that they had come to a confluence, where two streams united to form the river as previously known. Who could blame them in such a case if they followed the branch of which the navigation was the more easy? But we should condemn their proceeding as arbitrary if they declared that this branch alone deserved to be called the Nile, and therefore that it was quite needless to trace the course of the other. A stay-at-home critic, himself quite incapable of exploring, might without immodesty declare the work to be imperfect until the neglected branch had been explored, and might urge the task on others better qualified than himself to do it successfully. And so I have thought that I could

without immodesty give my opinion that the last word on the subject of New Testament criticism had not been said until the question of the origin of Western readings had received more attention than WH had been disposed to give it.

A reader of WH would have no other explanation suggested to him of the licentiousness of Western scribes than, to use a now current expression, that these scribes had been born with a double dose of original sin. It is the more hard to believe in so curious a geographical phenomenon because there does not seem to be historic ground for the opinion that Christians in the West were less solicitous than those in the East for preserving the purity of the sacred text. At the beginning of the third century the Roman presbyter Caius used language concerning the impiety of those who tampered with the Gospel text quite as strong as Burgon himself could have employed; and towards the end of the preceding century Irenæus is found using arguments which could have no force to one who did not believe in

the verbal inspiration of the Evangelists. So we are tempted to examine more closely an explanation that had been suggested of the fact that some things are attested by the vast majority of the MSS. of Luke's writings which we would gladly believe to be genuine, yet cannot understand how, if so, they could be omitted from certain very early MSS. which do not contain them. It has been suggested that Luke may have published two editions of his Gospel, adding in the second edition some supplementary statements which had been absent from the first. If this were so, we should be wrong in assuming the shorter text to be the only genuine one and in ascribing all additions to it to the licentiousness of transcribers. Both editions would be genuine, and the fuller one, as having the author's last care, would be the more valuable of the two. But the earliest witnesses who speak of our Gospels as being in circulation in their time are very many years too late to be able to give us authentic information as to the circumstances of their first publication. We can therefore

have no external evidence enabling us either to confirm or reject the hypothesis of a double edition.

Hort (p. 177) dismisses this hypothesis with scant consideration, finding that the readings affected by it have little note of originality, and in some cases the reverse. Bishop Lightfoot, however, had taken a more favourable view of this hypothesis. Speaking of the omission in some texts of the words addressed to James and John (Luke ix. 56), of the agony in the garden (xxii. 43, 44), and the solemn words on the cross (xxiii. 34), he says¹: "It seems impossible to believe that these incidents are other than authentic, and the solution will suggest itself that the Evangelist himself may have issued two separate editions. This conjecture will be confirmed by observing that in the second treatise of St. Luke similar traces of two editions are seen, where the passages omitted in many texts, though not important in themselves (*e.g.* Acts xxviii. 16, 29),²

¹ *Fresh Revision of English New Testament*, p. 29.

² These two verses have been ruthlessly cut out of the

bear equal evidence of authenticity, and are entirely free from suspicion on the ground that they were inserted to serve any purpose devotional or doctrinal." And a little time ago Blass, proceeding on the lines that Lightfoot had indicated, showed by a careful study of the Acts that the hypothesis of a double edition of that work deserves serious consideration ; and therefore that the hypothesis of a double edition of the Gospel cannot be summarily dismissed.

There is no document that has been thought more worthless for critical purposes than the

text of the Revised New Testament ; yet in the verse which tells that the centurion "delivered the prisoners to the captain of the guard," the latter word must have been read by the author of the translation preserved in the Gigas, which renders τῷ στρατοπεδάρχῃ, *principi peregrinorum*, on the meaning of which phrase see Harnack and Mommsen (*Berichte der Berl. Akad.*, 1895, 491). It is well worth while to read Mommsen's explanation who that officer was and why he was the very person to whom the charge of Paul was likely to be transferred by the centurion. This version must have been made while there was still a precise recollection at Rome of the officer in whose custody Paul had been placed. Naturally this was a point which might well be omitted from an edition intended for Easterns.

text of the Acts as given by Codex D. In a great number of places where the sense agrees with the Received Text, the wording appears to have been licentiously and causelessly altered; and, besides, there are several alterations affecting the sense, as well as additions to the Received Text, which it had seemed impossible to accept on so suspicious authority. But on a careful examination of these variations, Blass has come to the conclusion, which in my opinion he has fully proved, that some of them show such independent knowledge of the facts and the circumstances, that no satisfactory account can be given of the statements except that they rest on the authority of Luke.¹ And in some of the variations which have not this internal note of genuineness D does not stand alone,

¹ Blass has published a restoration of this Western version, using other authorities besides Codex D, in which he considers that the purity of the Western text has suffered by mixture with the other type. The witnesses, however, to this Western text are so scanty, and in places so defective, that while I am convinced that some things in that text certainly rest on apostolic authority, there are many more in Blass's restored text which do not so clearly commend themselves to me. If there were from the first

for some of its peculiarities are found in quotations from the Acts by early Latin Fathers.¹

Blass's account of the matter is that Luke, having made a rough draft of his history, desired to make a handsome copy of it for presentation to Theophilus. Not being rich enough to employ a professional scribe to make the copy, he had to make it himself, and naturally, instead of slavishly following his first draft, exercised the freedom which an author can lawfully take with a work of his own, in altering phraseology and lopping superfluities. From both forms of the work Blass imagines that

two authoritative texts, there was not only likely to arise a mixture of the two, but also a tendency of scribes to be less punctilious in adhering to the text of their archetype when once they had recognized that it had no exclusive authority.

¹ More recently Conybeare (*American Journal of Philology*) has found traces of the Western recension of the Acts in an Armenian translation of a commentary on the book by Ephraem Syrus. He finds also the curious result that the commentary on the Acts ascribed to Chrysostom appears to have been based on an older commentary, several of its explanations being based on the Western recension, which, however, was not used by the commentator himself.

copies were made ; the current text representing to us the copy made for Theophilus, while copies made from the original draft had some circulation in the West.

If we had to deal only with the Acts, I should look for no other explanation of the facts. But if the fact of a double edition of the Acts is established, it becomes probable that the like may be true of the Gospel ; and it does not seem *a priori* improbable that when Luke published the Acts he might also have published a revised edition of the Gospel. Blass has accepted the challenge to extend his theory to Luke's Gospel, of which also he holds that there were two editions, save that in this case he believes that the Received Text represents the earlier form published while Luke was still in the East, and the Western text the revised form published after the Evangelist had visited Rome. But clearly speculations concerning the Gospels and the Acts stand on a different footing ; both for other reasons, and because it is not possible in the former case, as it is in the latter, to establish

the conjecture by pointing out statements not contained in the Received Text which could scarcely have come from any hand but Luke's.

I therefore do not scruple to offer an alternative way of accounting for the phenomena which seems to me to deserve consideration—*viz.* that Luke may have continued to reside at Rome after the expiration of Paul's two years, and may there have given readings of his work ; and that explanatory statements which he then made were preserved in the West. It need hardly be mentioned that public recitation was a form of publication which prevailed in the days when Juvenal counted it as one of the plagues of Rome that even the month of August put no stop to the recitation of their works by poets. We may give no credence to the account that Herodotus read his history at the Olympian games ; but at the time when Lucian told the story that must have seemed a natural mode of publication. In fact, long after the art of writing came into use it must have remained a rare accomplishment, used rather for the preservation than the propagation of knowledge.

Grown men, like young children nowadays, liked to have stories read to them, before they were able to read them for themselves ; and there must have been a considerable use of the art of writing before there was any very general use of the art of reading. In the apostolic age Rome may be regarded as a literary city ; books were numerous, and not dear ; yet we may well believe that there was a large number of people who found it pleasanter to hear them read than to read them for themselves. In any case, we need not doubt that the great bulk of the early Christian community knew the Gospel history, not from the reading of the book, but from hearing it read Sunday after Sunday. We know, from the earliest authorities who tell us anything of the Christian weekly service, that the reading of the Gospel history formed part of it, and we need not doubt that before the Gospel was put into writing, the story was told by those best able to relate it.

I may mention without laying stress on it a speculation of my own, that the office of evangelists of whom the New Testament makes

mention without defining their special function may have been this telling of the Gospel story. Philip, who was called the Evangelist, was plainly a person who had had good opportunities for qualifying himself for such work. When Paul and Barnabas took Mark with them as their travelling companion, it may have been on account of his special fitness for this duty. Later Luke may have discharged the same office. The young Timothy might have shown quickness of apprehension and strength of memory such as to induce Paul to take him with him, and train him for this work of an evangelist which he is afterwards exhorted to fulfil.

However this may be, what I consider we ought to bear in mind is that the first publication of the Gospel story was oral and official. When I say official, I mean that if we take our Gospels to be embodiments of an oral tradition, it was not one formed by individual Christians writing down things which they had happened to hear from Apostles or other actual disciples of our Lord, but by their preserving the form

in which authorized teachers had weekly proclaimed it in the Church. Thus I reject the account of the genesis of our Gospels given by Renan, whose idea is that an originally meagre outline was filled in with stories which individual Christians had written, each in the margin of his own copy, according as they touched his heart. Such a conception is not appropriate to times when it is not to be assumed that Christians in general possessed pocket Bibles, in which they could make notes from time to time; and it is not to be thought probable that changes made by private authority could get any wide circulation. On the contrary, I believe that no changes took place in the Gospel text read in the public services except by the direction of the bishop or other presiding authority by whom the services were regulated. On the other hand, I believe, as I have already said, that with such direction moderate changes could easily be made.¹ Thus,

¹ On this account it seems to me on reflection that Burgon's explanation, which I once thought quite ridiculous, why the MSS. \aleph and B have survived to our time, may

though I have had difficulty in accepting Hort's hypothesis of a Syrian revision, when the changes seemed to be represented as effected by the influence of some unknown scholar, the hypothesis becomes credible to me if the reviser were supposed to have succeeded in inspiring with confidence some leading bishop.

The fact, however, remains to be accounted for that very early in the history of the Church there came to be differences between the Gospels as read at Rome and at Alexandria. But it is obvious that the conditions of learning the Gospel story must have been different in the two places. Alexandria was a city that

be accepted with some modification. These MSS. were evidently written for use in the public Church service. If they had continued to be so used, they would in due time have been hacked to pieces, and would now not reach us, except possibly in fragments as palimpsests. It is probable, therefore, that at some period of their existence they were withdrawn from Church use, and were chiefly preserved on account of their cost and beauty. Why they were withdrawn we cannot tell. It may have been because the bishop preferred the text current in his time; but it may have been merely because a smaller and less costly book was judged to be more convenient to be placed in the readers' hands.

did not much lie in the way of the earliest Christian preachers; and its knowledge of our Lord's history is likely to have been mainly derived from written records brought there by an early convert. Blass finds an indication of this in the Acts, and I am not prepared to dismiss his speculation as altogether baseless. In Acts xvii. 25 Apollos is described as speaking and teaching ἀκριβῶς the things concerning Jesus, but knowing only the baptism of John until Priscilla and Aquila expounded to him the way of God ἀκριβέστερον. Some time ago a learned lay friend proposed to me a conjectural emendation for the word ἀκριβῶς, and certainly there seemed need for emendation; for how could one who did not know about Christian baptism be said to know our religion accurately? The New Testament Revisers seem to have felt the difficulty; for they depart from their rule of translating as far as possible the same Greek words by the same English, and translate "carefully" the word ἀκριβῶς, which they translate "exactly" in the other places where it occurs in the Acts (xxiii. 15, 20,

xxiv. 22). But the word "carefully" is here not appropriate, for clearly the fault found with the teaching of Apollos is not want of diligence, but want of exact knowledge. And yet his knowledge was defective in a point which one would think must have been one of the first things the Christian who converted him must have taught him (see Acts ii. 38, viii. 16, 36). How then was it possible for Apollos to know accurately the story of our Lord, and yet not have heard of the necessity of being baptized in His name?

Blass's solution is, Apollos (or perhaps we should rather say his teacher) had not been in oral communication with any of the apostolic company, but had learned the religion from a book. If, for instance, the Gospel of St. Mark in the shorter form had already reached Alexandria, he might have learned from it accurately everything about our Lord's teaching, His mighty works, and His death, yet have learned nothing about the necessity of any baptism but John's. It would be very interesting if we could find reason to think

that a written Gospel could have reached Alexandria at so early a date as that of the conversion of Apollos. However that may be, it seems unlikely that any teacher could have reached Alexandria with authority such as to make him be thought capable of improving on the written records that had come to them; and in that literary city the form in which the Gospel had originally reached it would be likely to be preserved with little substantial alteration.

But it was quite otherwise with Rome, which from the very first was visited by men who had at least heard of our Lord from those who had actually conversed with Him, and soon after by men of the very highest authority in the Christian Church. Men who had first-hand knowledge of the facts were not likely to be left unquestioned; and in particular those two visitors to Rome, Luke and Mark, who had written Gospels, were likely to be asked for explanations, if anything in their writings seemed to need it; and these explanations would be likely to be preserved after they

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had gone, and to be read in the Church as authorized commentary on their writings. It must be remembered that official shorthand reporting was common at the time. Here again we come in contact with the Synoptic question; for one of the explanations which have been offered of the verbal differences between the Evangelists where they tell substantially the same story, is that they arise from the natural variations between the reports given by two different hearers of a story orally delivered in the presence of both. I am tempted to regard favourably this way of accounting for Western variations in St. Luke's Gospel, because I think that if there had been a definite Western written text we should have been able to reproduce it in a way that we cannot now.

It may not always be easy to distinguish between authorized and unauthorized commentary. Thus the verse (Acts xv. 34), "It pleased Silas to abide there still," gives an obvious explanation which might have occurred to any intelligent reader; but it may also be the answer given by Luke himself to the

question, "Did you not say that Silas had been sent away?" So again Acts viii. 37 may have been, as good critics think, a later interpolation; but it may also be that it gives Luke's answer to the question, And was the eunuch baptized without being asked for any profession of belief?¹ But when we find in the Western text of the Acts other less obvious explanations which imply knowledge not likely to be in possession of any one but the author himself, we are disposed to take a more favourable view of the cases which are open to doubt.

The same remark applies to the Gospel. Thus the words of our Lord's rebuke to James and John, when they asked leave to call down fire from heaven on a village of the Samaritans, have only Western authority, and therefore find no place in the text either of Tischendorf or

¹ If, as has been suggested, the story of a baptism without a profession of faith was likely to have given offence at Rome, a baptism with a profession so meagre, as compared with subsequent Church use, was equally likely to have given offence at Alexandria.

of WH. Yet though the words, "I am not come to destroy men's lives, but to save them," are such as we can well imagine a disciple of our Lord's attributing to his Master,¹ yet the preceding words, "Ye know not what manner of spirit ye are of," are far less obvious, and are more like tradition than invention. If Luke in reading his Gospel had merely told that our Lord had rebuked His disciples, curious hearers might naturally ask, What did He say?

Whatever we may think of this particular case, it seems to me a mistake to regard Western variations as licentious additions made by audacious scribes who did not scruple to

¹ It might be said that these words were suggested by John xii. 47, "I came not to judge the world, but to save the world" (see also John iii. 17). On the other hand, if the Western tradition is correct that our Lord addressed these words to John, it was natural that they should dwell in his memory, and make it natural for him to recall similar utterances. Even if it be denied that John wrote the fourth Gospel, in that case its author must have been much indebted to Luke's Gospel, and the existence of passages in the fourth Gospel might strengthen the case for their existence in the third.

insert in the Gospel text anything they happened to hear. On the contrary, many of them express, as I believe, the form in which the Gospel was read in the Church of Rome in apostolic or sub-apostolic times. To reject this form without examination, because it contained some things which did not find their way to Alexandria, is to shut our eyes to the fact that Rome was visited by teachers of the highest authority, able to speak of the facts from first-hand knowledge. We might as well praise a publisher who, having got hold of a first edition of Tennyson's poems, should now give to the world a pure text cleared of the alterations and additions by which subsequent editions had been corrupted.

Even the most licentious changes of all, such as we find in Codex D, where the words of one Gospel are boldly inserted in another, do not indicate disrespect for the authority of the Gospel thus arbitrarily dealt with, but were rather suggested by a sense of the co-ordinate and equal authority of all. It is men who have the strongest belief even in the verbal inspira-

tion of the books of Scripture, who, paying little attention to the human element in their authorship, and regarding God as alike the author of all, in their interpretations combine texts from different books, which a critic of a different school might regard as having no relation to each other. Such a work as Tatian's Diatessaron was probably called forth by the exigencies of missionary labour. When the Gospel story had to be told to a congregation with little previous knowledge of it, the question would arise, In what form shall it be told? which of our four Gospels shall we read for them? And it was a not unnatural decision to refuse to perplex the minds of an unlearned congregation with a variety of forms, but rather combine the four into one continuous Gospel. The same question that arose in Assyria would arise also in Southern Gaul; and the idea which Tatian afterwards completely carried out may have been either learned by him, or have been suggested by him to others, while he was still resident in the West.

CHAPTER VII.

CONCLUSION.

ON the whole I look on WH's conception that Western variations originated in the fancy of individual scribes to be quite akin to Renan's idea that the Gospels themselves originated in like manner from the contribution of individual Christians, whereas my belief is that the Gospels owe their authority not so much to the eminence of their authors as to the fact that they represent forms in which the story of our Lord's life was officially told in the Churches which His Apostles founded. The fact that we have four Gospels shows, when we reflect on it, that in different places the story was not told in exactly the same way, and therefore there is nothing to shock us if we find that even the text of each separate Gospel was not read in exactly the same way in one Church and in another.

The most striking difference is with respect to the conclusion of St. Mark's Gospel. The facts as to its circulation lead to the conclusions that it was not contained in the MS. which earliest reached Alexandria, and which was there regarded as the highest authority ; but that it *was* included in the form, in which from the earliest times that we can trace the history back, the Gospel was read in the Church of Rome ; and therefore I should be disposed to call the one the Roman, the other the Alexandrian form.¹ Before the beginning of the fourth century the Roman form had acquired such circulation all over the Christian world that it required the vigilant care of editors anxious for what they conceived to be the purity of the text to get a copy produced which did not contain the concluding verses.² I

¹ The idea suggested to Mr. Conybeare by a note in an Armenian MS. that the author of the Roman form was the Aristion mentioned by Papias is in itself improbable, while the evidence for it is exceedingly weak.

² Certainly in the Sinaitic, and probably in the Vatican MS. also, the verses would seem to have been originally copied and struck out on editorial revision.

think that critics will not ultimately acquiesce in Hort's view that this conclusion is a piece of an independent narrative which some one chose to append to St. Mark's Gospel, but will believe that it was from the first composed for the purpose for which it has served, at any rate since the time of Irenæus, to bring the second Gospel to a more satisfactory termination.

To sum up in conclusion, I have but to repeat my belief that what Westcott and Hort have restored is the text which had the highest authority in Alexandria in the third century, and may have reached that city in the preceding one. It would need but to strike out the double brackets from the so-called non-Western interpolations, and to remove altogether the few passages which WH reluctantly admitted into their pages with marks of doubt, when we should have a pure Alexandrian text. Their success is due to the fact that WH investigated the subject as a merely literary problem; and the careful preservation at Alexandria of a text which had reached that city was but a literary problem.

To illustrate what I mean by the distinction, I recur to an example already given. If the Alexandrian text really presents to us the original text of the first Gospel, it is important, from a literary point of view, to preserve the fact that the author was a person capable of calling two kings of Judah Asaph and Amos instead of Asa and Amon. We must attend to this fact in discussing the questions who this author was, when he lived, and whether or not he was St. Matthew. But the Revised English New Testament, being intended for Church use, though recognizing the faulty reading as that of the Greek, does not admit it into its text, but relegates it to the margin.

Thus I consider that, while Burgon and Miller exaggerated the ecclesiastical aspect of the question, Westcott and Hort do not attach sufficient value to the sanction given to a text by Church use. Burgon and Miller seem to hold that, no matter how long the Church had taken to make up her mind on the matter, the text which has at length been definitely selected

gains immunity from criticism, and if it has reigned for a thousand years cannot be departed from without impiety. I hold, on the contrary, that in critical science the rule *nullum tempus* prevails; that it is never too late to reverse a wrong decision; and that what was not recognized by the first age cannot refuse to submit its claims to scrutiny on the plea that it has enjoyed a usurped authority for a thousand years. But though I do not feel myself bound to conform to the use of the later Church, I cannot lightly regard the practice of the Church of the time when it was in immediate contact with men of the generation to which we owe the Gospels.

That WH should employ the Alexandrian "use" as their chief guide to the recovery of the original text may be quite right; but that they should refuse a place on their page to anything that has not that authority is an extreme which makes me glad that the Revised New Testament, which so closely follows their authority, has not superseded the Authorized Version in our churches. For, if it had, the

included by me

result might be that things would be accounted unfit to be read in the churches of the nineteenth century which were read at Rome in the second century, during the lifetime of men who had seen members of the apostolic company who had visited their city. If the Roman text were different from the Alexandrian, it might be only as the second edition of a book differs from the first. New Testament editors ought not to be infected with the fashionable craze for collecting first editions. A collector is said to have cooled an inexperienced friend's satisfaction with a purchase by the criticism, "You have got the bad edition. It has not got *les fautes* which are in the good edition."

But it may be said, This is all very well if you could be sure that the Western variations really have as high authority as the first published text; but what do you know with any certainty about their origin? In the first place, I would explain that I do not plead for the public reading of things which rest only on private authority, such as Clement's story of St. John and the robber, or the story of the man working

on the Sabbath ; nor even of passages which ultimately did obtain Church sanction, if that sanction were given late, as, for example, the section of the woman taken in adultery. And even with regard to the best attested Western variations it may be conceded that their right to rank as Scripture is matter only of probability, not of certainty.

I think I have said already that my chief hesitation in following WH is with respect to points which they and Burgon hold in common. On whatever points these critics differ, the work of both assumes the older doctrine of inspiration, according to which a sharp line of distinction is drawn between inspired Scripture, which is God's word, whereof every particle is infallibly true, and uninspired writings, the work of fallible men, which may be accepted or disbelieved at our discretion. But investigation convinces us that, instead of the light being separated from the darkness by a well-marked line of division, we find the one shading off into the other through a well-illuminated penumbra, as to which we may be in doubt on

which side it is to be reckoned. We find this to be the case with regard to the books of Scripture themselves. The evidence that St. Peter wrote the Second Epistle ascribed to him is not as strong as that Paul wrote the Epistle to the Galatians. With regard to the former Epistle, leading Christians in the fourth century differed in opinion, or were doubtful. If therefore we in the nineteenth century use all legitimate means to inform our judgment, we shall not endanger our salvation though in this matter we judge wrongly. How much less right then have we to complain if absolute certainty has not been vouchsafed to us as to whether or not an exceedingly small fraction of the third or fourth Gospel is rightly ascribed to Luke or John!

But, however reasonable it may be to ask for some suspension of judgment, I am well aware that he who makes such a proposal must expect great reluctance to grant it. If the subject is one on which people feel themselves incompetent to form a judgment of their own and need learned guidance, the first condition of

their putting confidence in their guide is that he shall put confidence in himself. If he is fully persuaded of his own ability to conduct them aright, they will follow him though he may lead them wrong, but they will not follow one who doubts and hesitates. Both Burgon and Hort have each the confidence necessary to gain adherents. The most I expect for my modest doubts is that they may stir up some better qualified person to the investigations necessary to enable him to speak more decisively.

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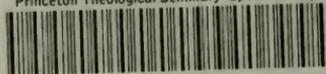
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